



**Health
Information
and Quality
Authority**

An tÚdarás Um Fhaisnéis
agus Cáilíocht Sláinte

Draft Guidelines for Evaluating the Clinical Effectiveness of Health Technologies in Ireland

2011

Safer Better Care

About the Health Information and Quality Authority

The Health Information and Quality Authority is the independent Authority established to drive continuous improvement in Ireland's health and social care services.

The Authority's mandate extends across the quality and safety of the public, private (within its social care function) and voluntary sectors. Reporting directly to the Minister for Health, the Health Information and Quality Authority has statutory responsibility for:

Setting Standards for Health and Social Services — Developing person-centred standards, based on evidence and best international practice, for health and social care services in Ireland (except mental health services)

Social Services Inspectorate — Registration and inspection of residential homes for children, older people and people with disabilities. Inspecting children detention schools and foster care services.

Monitoring Healthcare Quality — Monitoring standards of quality and safety in our health services and investigating as necessary serious concerns about the health and welfare of service users

Health Technology Assessment — Ensuring the best outcome for the service user by evaluating the clinical and economic effectiveness of drugs, equipment, diagnostic techniques and health promotion activities

Health Information — Advising on the collection and sharing of information across the services, evaluating information and publishing information about the delivery and performance of Ireland's health and social care services

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Foreword

The Health Information and Quality Authority (the Authority) has a statutory remit to evaluate the clinical and cost-effectiveness of health technologies. Arising out of these evaluations the Authority provides advice to the Minister for Health and to the Health Service Executive (HSE). The primary audience for health technology assessments (HTAs) conducted by the Authority is therefore decision makers within the publicly-funded healthcare system. It is also recognised that the findings of a HTA may have implications for other key stakeholders in the Irish healthcare system. These include patient groups, the general public, clinicians, other healthcare providers, academic groups and the manufacturing industry.

The HTA guidelines provide an overview of the principles and methods used in assessing health technologies. They are intended as a guide for all those who are involved in the conduct or use of HTA in Ireland. The purpose of the guidelines is to promote the production of assessments that are timely, reliable, consistent and relevant to the needs of decision makers and key stakeholders in Ireland.

The HTA guidelines will comprise several sections including guidance on economic evaluation, budget impact analysis, social, ethical and organisational aspects of HTA and recommended reporting formats. Each of these sections is important. For ease of use the sections of the guidelines are being developed as stand-alone documents.

This document, *Draft Guidelines for Evaluating the Clinical Effectiveness of Health Technologies in Ireland*, is the third in the series and complements the *Guidelines for Economic Evaluation of Health Technologies in Ireland* (2010) and the *Guidelines for Budget Impact Analysis of Health Technologies in Ireland* (2010). This document is limited to methodological guidance on the evaluation of clinical effectiveness and is intended to promote best practice in this area. The guidelines will be reviewed and revised as necessary.

The purpose of these guidelines is to assist those conducting or using HTA in Ireland. They are intended to inform technology assessments conducted by, or on behalf of the Health Information and Quality Authority, the National Centre for Pharmacoeconomics, the Department of Health and the Health Service Executive (HSE), to include health technology suppliers preparing applications for reimbursement.

The guidelines are intended to be applicable to all healthcare technologies, including pharmaceuticals, procedures, medical devices, broader public health interventions and service delivery models. For ease of use, guideline statements that summarise key points are included prior to each section in italics.

The draft Guidelines for evaluating clinical effectiveness have been developed in consultation with the Scientific Advisory Group of the Authority. Providing broad representation from key stakeholders in healthcare in Ireland, this group includes methodological experts from the field of HTA. The Authority would like to thank the members of the Scientific Advisory Group and its Chairperson, Dr Michael Barry from

the National Centre for Pharmacoeconomics, and all who have contributed to the production of these draft Guidelines.

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Process and Acknowledgements

This document, *Draft Guidelines for Evaluating the Clinical Effectiveness of Health Technologies in Ireland* is a complementary document to the *Guidelines for Economic Evaluation of Health Technologies in Ireland* (2010) and to the *Guidelines for Budget Impact Analysis of Health Technologies in Ireland* (2010). They are limited to methodological guidance on the evaluation of clinical effectiveness and are intended to promote best practice in this area. They will be reviewed and revised as necessary, with updates provided online through the Authority's website (www.hiqa.ie). Each of the documents listed above will form part of a series of guidelines for health technology assessment (HTA) in Ireland that the Authority will develop and continuously review in the coming years.

Each of the guidelines in the series were and will continue to be developed by the Authority with technical input from the National Centre for Pharmacoeconomics and in consultation with its Scientific Advisory Group (SAG). Providing broad representation from key stakeholders in Irish healthcare, this group includes methodological experts from the field of HTA. The group provides ongoing advice and support to the Authority in its development of national HTA guidelines. The terms of reference for this group are to:

- contribute fully to the work, debate and decision-making processes of the Group by providing expert technical and scientific guidance at SAG meetings as appropriate
- be prepared to occasionally provide expert advice on relevant issues outside of SAG meetings, as requested
- support the Authority in the generation of guidelines to establish quality standards for the conduct of HTA in Ireland
- support the Authority in the development of methodologies for effective HTA in Ireland
- advise the Authority on its proposed HTA Guidelines Work Plan and on priorities as required
- support the Authority in achieving its objectives outlined in the HTA Guidelines Work Plan
- review draft guidelines and other HTA documents developed by the Authority and recommend amendments as appropriate
- contribute to the Authority's development of its approach to HTA by participating in an evaluation of the process as required.

Following review by the SAG, the draft guidelines will be made available for broader consultation. Feedback will be obtained by open consultation through the Authority's website and by targeted consultation with key stakeholders in Irish healthcare.

The membership of the Scientific Advisory Group is as follows:

Chairperson: Dr Michael Barry,
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Contributors

The Authority gratefully acknowledges all those who contributed to the development of these draft Guidelines.

Record of Updates

Date	Title / Version	Summary of changes

Draft Guidelines for Evaluating the Clinical Effectiveness of Health Technologies in Ireland

Issued:

This document is one of a set that describes the methods and processes for conducting health technology assessment in Ireland.

The document is available from the HIQA website (www.hiqa.ie).

List of Abbreviations

HIQA	Health Information and Quality Authority
HSE	Health Service Executive
HTA	health technology assessment
IPD	individual patient data
ITT	intention-to-treat
MTC	mixed treatment comparison
QALY	quality-adjusted life years
PRO	patient-reported outcome
RCT	randomised controlled trials
SAG	Scientific Advisory Group

1 Introduction

Health Technology Assessment (HTA) is a multi-disciplinary activity that delivers input for prioritising and decision-making in the health care system in relation to prevention, diagnostics, treatment and rehabilitation. HTA deals with the reasons - scientific, economic, ethical, and social - for adopting a particular medical technology, practice, or system of care and how such adoption or non-adoption influences both the distribution of resources and the quality of services within the health care infrastructure. The purpose of HTA is to inform health policy decisions that promote safe, effective, efficient, patient-focussed healthcare.

The primary audience for HTAs in Ireland is decision makers within the publicly-funded health and social care system. It is recognised that the findings of a HTA may also have implications for other stakeholders in the system. Stakeholders include patient groups, the general public, clinicians, other healthcare providers, academic groups and the manufacturing industry.

The Authority continues to develop a series of methodological guidelines that are intended to assist those that conduct HTA for or on behalf of the Health Information and Quality Authority, the National Centre for Pharmacoeconomics, the Department of Health and the Health Service Executive. They should, additionally, prove invaluable to technology sponsors preparing applications for reimbursement. Their purpose is to promote the production of assessments that are timely, reliable, consistent and relevant to the needs of decision makers and other stakeholders.

The series of guidelines are intended to be applicable to all healthcare technologies, including pharmaceuticals, procedures, medical devices, broader public health interventions, and service delivery models. They are therefore broad in scope and some aspects may be more relevant to particular technologies than others.

The Draft Clinical Effectiveness Guidelines represent one component of the overall series. They are limited to the methodological guidance on the evaluation of the clinical effectiveness of technologies in HTA. These draft Guidelines are intended to be viewed as a complementary document to the existing economic guidelines and Budget Impact Analysis (BIA) of Health Technologies in Ireland..

The content of this document was partly derived from text prepared by the Authority for inclusion in an overarching HTA guideline being prepared by the EUnetHTA collaboration. These draft Guidelines have drawn on published research and will be reviewed and revised as necessary following consultation with the various stakeholders, including those in the Scientific Advisory Group.

1.1 Clinical effectiveness guidelines

Clinical effectiveness describes the ability of a technology to achieve a clinically significant impact on a patient's health status. The evaluation of clinical effectiveness is considered in this document under two headings: measures of effect and methods of comparison or meta-analysis. The former are used to determine the impact of a technology on a patient's health status. There are numerous methods available to measure and report treatment effects and many associated methodological issues. Measures of effect are discussed in section 2 of this document.

To compare two or more technologies, the measured effects of those technologies are often combined across a number of studies to maximise the evidence base. Data from multiple studies are typically combined in a meta-analysis. There are a variety of meta-analysis methodologies available that are appropriate in different contexts. Section 3 of this document outlines the methods of meta-analysis available, the main issues and considerations associated with meta-analysis and provides guidance on selecting the most appropriate method for a given analysis.

In this document, the descriptions of type of effect measure and method of meta-analysis follow a standard format using the following set headings: description, examples, usage, strengths, limitations, and critical questions.

1.2 Document layout

For ease of use, a list of the guideline statements that summarise the key points of the guidance is included at the end of this chapter. These guideline statements are also included in italics at the beginning of each section for the individual elements described in chapters 2 and 3.

1.3 Explanation of terms

A number of terms used in the draft Guidelines may be interpreted more broadly elsewhere or may have synonymous terms that could be considered interchangeable. The following outlines the specific meanings that may be inferred for these terms within the context of these Guidelines and identifies the term that will be used throughout the Guidelines for the purpose of consistency.

'Technology' includes any intervention that may be used to promote health, to prevent, diagnose or treat disease, or that is used in rehabilitation or long-term care. This includes: pharmaceuticals, devices, medical equipment, medical and surgical procedures, and the organisational and supportive systems within which healthcare is provided. Within the context of these draft Guidelines the terms 'intervention' and 'technology' should be considered to be interchangeable, with the term 'technology' used throughout for the purpose of consistency.

Efficacy is the extent to which a treatment has the ability to achieve the intended effect under ideal circumstances. Effectiveness is the extent to which a treatment achieves the intended effect in the typical clinical setting. Efficacy studies usually

precede effectiveness studies. Both efficacy and effectiveness studies provide valuable information about treatment effect.

1.4 Summary of guideline statements for measures of effect

Types of data (Section 2.1.1) Endpoints can be expressed as continuous, categorical or count data. When continuous data are expressed as categorical, the selection of cut-points must be clearly described and justified.

Relative and absolute endpoints (Section 2.1.2) Absolute measures are presented as a difference and are dependent on the baseline risk in the study population. Relative measures are presented as a ratio and are independent of the baseline risk. Endpoints should be expressed in both absolute and relative terms where possible.

Efficacy and effectiveness (Section 2.1.3) Efficacy is the extent to which a treatment has the ability to achieve the intended effect under ideal circumstances. Effectiveness is the extent to which a treatment achieves the intended effect in the typical clinical setting. Both efficacy and effectiveness studies provide valuable information about treatment effect. Where available, both efficacy and effectiveness must be reported.

Endpoint reliability and validity (Section 2.1.4) A reliable endpoint returns the same value with repeated measurements. A valid endpoint accurately measures the endpoint it was intended to measure. Endpoints used in an assessment must have demonstrated reliability and validity.

Internal and external validity of a trial (Section 2.1.5) Internal validity is the extent to which bias is minimised in a trial. External validity is the extent to which the findings of the trial can be generalised to other settings or populations. Treatment effect should be measured in trials that have both internal and external validity.

Time to event data (Section 2.1.6) In survival analysis, overall survival should be considered the gold standard for demonstrating clinical benefit. In assessing progression-free, relapse-free and disease-free survival, patients must be evaluated on a regular basis to ensure that the time of progression is measured accurately. The length of follow-up must be clearly defined and relevant to the disease in question. It should be clear whether all or only the first post-treatment event was recorded.

Multiple endpoints (Section 2.1.7) All relevant endpoints used in the literature should be reported in an assessment.

Subgroup analysis (Section 2.1.8) Consideration should be given to the inclusion of eligible subgroups that have been clearly defined and identified based on an a priori expectation of differences, supported by a plausible biological or clinical rationale for the subgroup effect.

Types of endpoint (Section 2.2) An endpoint must be clearly defined and measurable. It must be reliable and valid. An endpoint should be relevant to the condition being treated and sensitive to change.

Patient-reported outcomes (PROs) (Section 2.2.1) PROs should be used to measure changes in health and functional status that are of direct relevance to the patient. A PRO should be sensitive to changes in health status. If a multi-dimensional measure is used, it should be clearly stated whether all or some of the dimensions were evaluated. The PRO should encompass domains relevant to the illness being treated. The use of mapping from one PRO to another must be clearly stated and justified. Only a validated mapping function based on empirical data should be used. The statistical properties of the mapping function should be clearly described. All PROs collected in a study should be reported.

Clinical endpoints (Section 2.2.2) The choice of clinical endpoint must be justified on the basis of a clear link between the disease process, technology and endpoint.

Surrogate endpoints (Section 2.2.3) A surrogate endpoint must have a clear biological or medical rationale or have a strong or validated link to a final endpoint of interest. The magnitude of the effect on the surrogate should be similar to that on the final endpoint.

Composite endpoints (Section 2.2.4) A change in composite endpoints should be clinically meaningful. All of the individual components of a composite must be reliable and valid endpoints. The components that drive the composite result should be identified.

Adverse outcomes (Section 2.2.5) All adverse effects that are of clinical or economic importance must be reported. Both the severity and frequency of harms should be reported. It must be clear whether harms are short-term or of lasting effect.

Sensitivity and specificity (Section 2.2.6) The sensitivity and specificity of a diagnostic or screening test should be measured in relation to a recognised reference test. The threshold for a positive test result should be clearly defined.

1.5 Summary of guideline statements for methods of meta-analysis

Gathering evidence (Section 3.1.1) The methods used to gather evidence for a meta-analysis must be clearly described. Evidence is typically gathered using a systematic review.

Individual patient data (Section 3.1.2) Individual patient data can be analysed in a meta-analysis. Individual patient data meta-analysis should not be used to the

exclusion of other relevant data. Results should be compared to a study-level analysis.

Types of study (Section 3.1.3) Evidence to support the effectiveness of a technology should be derived by clearly defined methods. Where available, evidence from high quality RCTs should be used to quantify efficacy.

Data and study quality (Section 3.1.4) Studies included in a meta-analysis should be graded for quality of evidence. The quality of evidence should be clearly stated. The results of a meta-analysis should be reported according to relevant standards.

Heterogeneity (Section 3.1.5) Heterogeneity of treatment effect between studies must be assessed. Where significant heterogeneity is observed, attempts should be made to identify its causes. Substantial heterogeneity must be dealt with appropriately and may preclude a meta-analysis.

Meta-regression (Section 3.1.6) When there is significant between-study heterogeneity, meta-regression is a useful tool for identifying study-level covariates that modify the treatment effect.

Fixed and random effects (Section 3.1.7) The choice between a fixed and random effects analysis is context specific. Heterogeneity should be assessed using standard methods. Significant heterogeneity suggests the use of a random effects model. Justification must be given for the choice of fixed or random effects model.

Sources of bias (Section 3.1.8) Attempts should be made to identify possible sources of bias such as publication bias, sponsorship bias and bias arising from the inclusion of poor quality studies. Potential sources of bias must be reported along with steps taken to minimise the impact of bias.

Frequentist and Bayesian approaches (Section 3.1.9) Both frequentist and Bayesian approaches are acceptable in meta-analysis. The approach taken must be clearly stated.

Outliers and influential studies (Section 3.1.10) Influential studies and those that are statistical outliers should be identified and reported. The methods used for identifying outliers must be clearly stated. Studies that are outliers should be characterised to determine if they are comparable to the other included studies.

Sensitivity analysis (Section 3.1.11) If potential outliers have been identified, or if plausible subgroups of patients or studies have been identified, a comprehensive sensitivity analysis should be conducted. In a Bayesian analysis the choice of priors should be tested using a sensitivity analysis.

Networks of evidence (Section 3.2) The network of available evidence should be described and used to guide the selection of the method of meta-analysis. The selection of direct and indirect evidence must be clearly defined. The exclusion of

relevant evidence, either direct or indirect, should be highlighted and justified. Where direct and indirect evidence are combined, inconsistencies between the direct and indirect evidence must be assessed and reported.

Selecting the method of comparison (Section 3.3) The choice of method of comparison depends on the quality, quantity and consistency of direct and indirect evidence. The available evidence must be clearly described along with a justification for the choice of method.

Methods of meta-analysis (Section 3.4) For any method of meta-analysis all included trials must be sufficiently comparable and measuring the same treatment effect.

Direct meta-analysis (Section 3.4.1) Direct meta-analysis should be used when there are sufficient comparable head to head studies available. If indirect evidence is available, then consideration should be given to a multiple treatment comparison.

Unadjusted indirect comparison (Section 3.4.2) The method of unadjusted indirect comparisons should not be used.

Adjusted indirect comparison (Section 3.4.3) Adjusted indirect comparison is appropriate for comparing two technologies using a common comparator.

Network meta-analysis (Section 3.4.4) A network meta-analysis is appropriate for analysing a combination of direct and indirect evidence where there is at least one closed loop of evidence connecting the two technologies of interest.

Bayesian mixed treatment comparison (Section 3.4.5) A Bayesian mixed treatment comparison is appropriate for comparing multiple treatments using both direct and indirect evidence.

Meta-analysis of diagnostic test accuracy studies (Section 3.4.6) The bivariate random effects and hierarchical summary receiver operating characteristic models (HSROC) should be used for pooling sensitivity and specificity from diagnostic and screening test accuracy studies. The correlation between sensitivity and specificity should be reported.

Generalised linear mixed models (Section 3.4.7) Generalised linear mixed models are appropriate when analysing individual patient data from trials.

Confidence profile method (Section 3.4.8) The confidence profile method can be used to combine direct and indirect evidence. Network meta-analysis or Bayesian mixed treatment comparison should be used in preference to the confidence profile method. The use of this method over other available methods should be justified.

2 Measures of effect

Measures of effect are used to determine treatment impact in terms of changes in health status. That impact is usually in the form of improved health status (e.g. survival, cure, remission), but it can also be worsening health status (e.g. adverse reactions, hospitalisations, deaths). Measures of effect should be clearly relevant to the disease, condition, complaint or process of interest. It should be possible to diagnose and interpret them, and they should be sensitive to treatment differences. Effects may be observed for any technology such as pharmaceutical, surgical, or therapeutic.

In these draft Guidelines measures of effect are referred to as endpoints. This chapter will describe issues that are relevant to a variety of endpoint types before describing types of endpoint.

2.1 Common considerations when assessing endpoints

There are a number of common important considerations that must be taken into account when assessing endpoints.

2.1.1 Types of data

Endpoints can be expressed as continuous, categorical or count data. When continuous data are expressed as categorical, the selection of cut-points must be clearly described and justified.

Endpoint data can be expressed in a variety of ways⁽¹⁾:

- Continuous - a continuous variable has numeric values (e.g. 1, 2, 3). The relative magnitude of the values is significant (e.g. a value of 2 indicates twice the magnitude of 1). Examples include blood pressure and prostate specific antigen.
- Categorical - a categorical variable classifies a subject into one of 2 or more unique categories (e.g. disease status - remission, mild, moderate, or severe relapse). A binary variable is a categorical variable with only two levels (e.g. mortality, stroke). An ordinal variable is a categorical variable that can be rank-ordered (e.g. self-reported health status, Clinical Global Impression).
- Count data - variables in which observations can only have non-negative integer values (e.g. number of hospitalisations).

Many endpoints are reported as proportions or rates and hence are continuous data. The techniques available for summarising and analysing the endpoint data are affected by how the data are expressed. Conversion of a variable from continuous to categorical results in the loss of information. The quality of the conversion depends on how homogeneous the observations are within each category. However, a categorical variable, particularly if binary, is often simpler to interpret.

Categorical endpoints, particularly when expressed in binary form, can be open to manipulation when derived from a continuous measure. For example, the threshold for distinguishing between healthy and ill in an endpoint can be set to show a treatment in the best light if there is no commonly agreed cut-off. Dichotomising does not introduce bias if the split is made at the median or some other pre-specified percentile. However, if the cut-point is chosen based on analysis of the data, in particular by splitting at the value which produces the largest difference in endpoints between categories, then severe bias will be introduced. Endpoints that are binary by nature, such as myocardial infarction, may still vary considerably in clinical interpretation.⁽²⁾

2.1.2 Relative and absolute endpoints

Absolute measures are presented as a difference and are dependent on the baseline risk in the study population. Relative measures are presented as a ratio and are independent of the baseline risk. Endpoints should be expressed in both absolute and relative terms where possible.

The endpoints of a trial can typically be expressed in absolute or relative terms. Absolute measures are presented as a difference and are dependent on the baseline risk in the study population. Relative measures are presented as a ratio and are independent of the baseline risk.

Absolute risk measures:

- are generally useful to clinicians as they provide a more realistic quantification of treatment effect than relative measures⁽³⁾
- have limited generalisability due to their dependence on baseline risk
- should not be pooled in a meta-analysis due to fact that the variation in baseline risk is not accounted for⁽⁴⁾
- cannot be applied to different subgroups unless they have been explicitly calculated for those subgroups
- examples include absolute risk reduction and number needed to treat

Relative risk measures:

- are usually stable across populations with different baseline risks and are useful when combining the results of different trials in a meta-analysis
- do not take into account a patient's risk of achieving the intended endpoint without the treatment and thus do not give a true reflection of how much benefit the patient would derive from the treatment⁽⁵⁾
- can be applied to different subgroups with the understanding that baseline risk will vary by subgroup and ignoring that subgroup characteristics may modify the treatment effect
- examples include relative risk, odds ratio and hazard ratio

The choice between absolute and relative is sometimes made to maximise the perceived effect. In some instances the absolute risk difference will be small whereas the relative risk might be large.⁽⁶⁾ Wherever possible both relative and absolute

measures should be presented as together they provide the magnitude and context of an effect.⁽⁷⁾ If both are not included then justification of the presented measure must be included.

2.1.3 Efficacy and effectiveness

Efficacy is the extent to which a treatment has the ability to achieve the intended effect under ideal circumstances. Effectiveness is the extent to which a treatment achieves the intended effect in the typical clinical setting. Both efficacy and effectiveness studies provide valuable information about treatment effect. Where available, both efficacy and effectiveness must be reported.

Efficacy is the extent to which a treatment has the ability to achieve the intended effect under ideal circumstances. Effectiveness is the extent to which a treatment achieves the intended effect in the typical clinical setting. Efficacy studies usually precede effectiveness studies.

Efficacy studies tend to favour condition-specific endpoints with strong links to the mechanism of action. They also tend to be collected in a short-term time horizon. Efficacy studies will sometimes have stringent exclusion criteria to prevent enrolling patients who are less likely to observe a significant treatment effect (e.g. those with co-morbidities, lower risk patients). Efficacy is generally measured in randomised controlled trials (RCTs).

Effectiveness studies tend to collect more comprehensive endpoint measures that reflect the range of benefits expected from the treatment that are relevant to the patient and to the payer, including improvement in ability to function and quality of life. These measures often have a weaker link to the mechanism of action. Both short- and longer-term horizons are typically considered in effectiveness studies. Effectiveness studies can be useful in identifying the true benefit of a technology in a real world or community setting. Examples of effectiveness studies include pragmatic RCTs, observational cohorts or registry data. Pragmatic RCTs, such as those nested in population based screening programmes, can generate high quality effectiveness data.

Efficacy does not necessarily correlate with effectiveness. The distinction between efficacy and effectiveness may be more pronounced for some endpoints, particularly endpoints that are sensitive to individual-level factors (e.g. co-morbidities, smoking status). Factors that may impact on the effectiveness of a treatment, such as adherence, should be documented.⁽⁸⁾

In clinical trials the analysis for efficacy and effectiveness are referred to as intention to treat (ITT) and per protocol, respectively. Intention-to-treat analysis patients are analysed according to the group into which they were randomised irrespective of whether or not they received that treatment. Per-protocol analysis, on the other hand, only considers the patients who fully adhered to the clinical trial instructions as

specified in the protocol. Intention-to-treat provides a more realistic view of how technologies work in practice whereas effect estimates from a per-protocol trial may be biased by non-random loss of patients. However, ITT may indicate acceptability for a technology not captured in the per protocol analysis.

Missing data can pose problems where outcomes are not recorded due to loss to follow up. There are a number of techniques for estimating values for missing data. The success of these techniques depend on whether or not the data are missing at random. One method is last observation carried forward (LOCF) and related methods baseline and worst observation carried forward (BOCF and WOCF, respectively). These methods impute the missing data using previous observations for a patient. Alternative methods such as multiple imputation (MI) and mixed-effect model for repeated measures (MRMM) also exist. LOCF methods tend to result in inflated rates of Type I errors compared to MI and MRMM methods.^(9;10) Preference is for the use of MI and MRMM methods. For dichotomous outcomes the non-responder imputation (NRI) method can be used, which assumes that all drop-outs are non-responders. Where missing data have been imputed, the method used should be clearly stated.

2.1.4 Endpoint reliability and validity

A reliable endpoint returns the same value with repeated measurements. A valid endpoint accurately measures the endpoint it was intended to measure. Endpoints used in an assessment must have demonstrated reliability and validity.

Endpoints should be both reliable and valid. Reliability refers to whether repeated measurements return the same value. Differences can arise due to the individual who takes the measurement (inter-rater reliability), the instruments used to make the measurements, or the context in which the measurement is made.⁽¹¹⁾ The reliability of the instruments used can be checked using test-retest reliability whereby the measurement is repeated and differences compared. The measures should be at an interval over which no change is expected. Particularly for subjective measures, the inter-rater reliability should be investigated. Depending on the subjectivity of the measure, substantial variability may occur across clinical or patient raters.

Validity refers to how accurately an instrument measures the endpoint it was intended to measure. There are a number of forms of validity:

- Construct validity – how well the endpoint represents reality in terms of cause and effect
- Content validity – how well the endpoint measures what it is intended to measure
- Criterion validity – how well the endpoint compares to a reference or gold-standard measure
- Face validity – if the endpoint appears to be valid to the clinician, patient or assessor

Direct measures of objective endpoints are presumed to have validity. Clearly, any subjectively measured endpoint must have established validity as shown in independent empirical studies.

Reliability and validity are not independent as an endpoint cannot be valid if it is not reliable, but could be reliable without being valid.⁽¹²⁾ If an endpoint is unreliable then it will not return the same value on repeated measures and hence will not have criterion validity. However, a reliable endpoint would not be valid if, for example, it is not related to the effect of the technology being assessed.

2.1.5 Internal and external validity of a trial

Internal validity is the extent to which bias is minimised in a trial. External validity is the extent to which the findings of the trial can be generalised to other settings or populations. Treatment effect should be measured in trials that have both internal and external validity.

Internal validity is concerned with the extent to which bias is minimised in a trial. A number of types of bias can impact on internal validity⁽¹³⁾:

- selection bias due to biased allocation to trial arms
- performance bias due to unequal provision of care
- detection bias due to biased endpoint assessment
- attrition bias due to loss to follow up

Internal validity can be maximised by a combination of careful study design, conduct and analysis. Proper randomisation prevents allocation bias. Endpoint measurement is prone to detection bias if adequate blinding has not been used in a study.⁽¹⁴⁾ Proper blinding of patients and clinicians can reduce or eliminate performance and detection bias. Blinding is particularly important for more subjective endpoint measures. Bias can also be introduced by systematic withdrawals or exclusions from the trial for patients receiving the technology. Maximising response rates in all study arms will reduce attrition bias. From an analytical point of view, it is important to know how a study has dealt with drop-outs and missing data when computing summary effect measures. Failure to properly account for poor response rates or missing data will introduce further bias into an analysis. If the internal validity of a trial is doubtful then the measured treatment effect must be questioned.

The external validity of trial impacts on the extent to which the findings of the trial are generalisable to other settings or populations. The main factors that impact on external validity are:

- patient profile (e.g. age-sex distribution, disease severity, risk factors, co-morbidity)
- treatment regimen including dosage, frequency and comparator treatment
- setting, for example primary, secondary or tertiary care
- endpoints (e.g. definition of endpoints, length of follow up)
- participation rate, as a poor participation rate may mean that the trial population is not representative of the target population

The number of patients required to achieve a given statistical power for a study is a function both of the risk in the control group, and of the hypothesised reduction in the risk due to treatment. For rarer endpoints the required sample size is larger. By enrolling high-risk patients trials can be run with a smaller sample size. It is also noted that many studies do not report the power and sample size calculations or whether they are testing for superiority, inferiority or equivalence which will impact on the ability to detect a statistically significant difference. The power and sample size should be appropriate for the type of test being carried out.

External validity can be maximised by ensuring that the trial characteristics closely match those found in routine clinical practice. The patients should be typical of those who would generally be eligible for the type of treatment being assessed. The treatment regimen should reflect what would realistically be provided in routine practice in terms of dose, frequency, adherence and compliance. The technology should be applied in a similar setting to routine practice and the measured endpoints should be those that are commonly accepted as relevant to the disease being treated. A lack of external validity does not imply that the measured treatment effect is incorrect but prevents the effect estimate from being generalised to other populations.

2.1.6 Time to event data

In survival analysis, overall survival should be considered the gold standard for demonstrating clinical benefit. In assessing progression-free, relapse-free and disease-free survival, patients must be evaluated on a regular basis to ensure that the time of progression is measured accurately. The length of follow-up must be clearly defined and relevant to the disease in question. It should be clear whether all or only the first post-treatment event was recorded.

Survival analysis measures when the endpoint occurred as well as if the endpoint occurred. Survival is a direct clinical benefit to patients. Common survival endpoints include overall survival, disease-free survival, relapse-free survival and progression-free survival. Overall survival is the gold standard for demonstrating clinical benefit and as such should be used when the primary aim of a technology is to extend life. Defined as the time from randomisation to death, this endpoint is unambiguous and is not subject to investigator interpretation. In assessing progression-free, relapse-free and disease-free survival, patients must be evaluated on a regular basis to ensure that the time at which a change in health status occurs is measured accurately. Where possible, both the overall survival rate and the intermediate data (e.g. progression-free, relapse-free and disease-free survival) should be reported, if available. When intermediate data are reported, they must be clearly defined.

Survival can also be expressed in terms of a hazard ratio which is a widely used metric to compare survival in two groups.⁽¹⁵⁾ The hazard ratio gives the relative risk of an endpoint at any given time with a value of 1 corresponding to equal hazards.

The ratio is based on the entire study period assuming that the ratio does not change through the study period. With a large enough sample size it is possible to calculate the hazard ratio for smaller time periods during a study.

A key issue in survival analysis is censoring – ceasing observation at the end of the study period.⁽¹⁶⁾ The length of follow-up should be explicitly stated and justification provided as to its relevance to the disease in question. Different studies may use quite different follow-up periods, rendering their findings incompatible. Data analysis cut-off dates and schedule of assessment have an impact on the probability of observing events related to the time frame. Incomplete reporting has been shown to be common, affecting the definition of survival terms and the numbers of patients at risk. It should be clear whether only the first post-treatment event was recorded or if all non-fatal events were recorded in the follow-up period.

2.1.7 Multiple endpoints

All relevant endpoints used in the literature should be reported in an assessment.

The use of multiple endpoints can give rise to Type 1 error whereby the probability of false-positive findings by chance is increased. A single primary endpoint and multiple secondary endpoints should be defined in the study protocol and consideration should be given to adjusting for multiple testing.⁽¹⁵⁾ In reality there may be multiple primary endpoints which may include safety endpoints. If multiple endpoints are included they should be justified. Reported endpoints may not be per protocol – in many instances, studies put forward the endpoint(s) where the most significant effect was observed.⁽¹⁷⁾ There is debate as to whether or not secondary endpoints should even be reported if the effect on the primary endpoint is not significant.⁽¹⁸⁾ To reduce reporting bias, all relevant endpoints used in the literature should be reported in an assessment.⁽¹⁹⁾ Where multiple endpoints are used they should be specified in advance and not selected on a post-hoc basis.

2.1.8 Subgroup analysis

Consideration should thus be given to the inclusion of eligible subgroups that have been clearly defined and identified based on an a priori expectation of differences, supported by a plausible biological or clinical rationale for the subgroup effect.

Subgroup analysis may be essential where there are potentially large differences in patient characteristics or treatment benefit that may be observed between groups. Subgroups should have been defined *a priori* with plausible reasons for expecting different treatment effects across subgroups. The use of subgroups increases the number of statistical tests undertaken and hence increases the chance of generating false-positive results. Trials need to be suitably powered for subgroup analysis and

this should be taken into account when analysing trial data. A test for interaction should be used to help determine if subgroups differ in their treatment effect.⁽²⁰⁾

A subgroup analysis may be required if the licensed indication is narrower than the indications included for the entire study population. In that instance it may be possible to restrict the analysis to the subgroup of patients treated for the licensed indication.

2.2 Types of endpoint

An endpoint must be clearly defined and measurable. It must be reliable and valid. An endpoint should be relevant to the condition being treated and sensitive to change.

The choice of endpoints used in a study or comparison will be influenced by the purpose for which they are measured.⁽²¹⁾ For example, if the primary purpose of a technology is to improve survival then mortality will be the relevant endpoint. If, however, a technology is designed to improve mobility then functional status may be a more appropriate endpoint.

This section looks at different endpoints types that have distinct modes of collection or purpose. For each type of endpoint there is a brief description, some typical examples, a brief note on usage in the literature, the strengths and limitations of that type of endpoint and then some critical questions that should be asked when assessing such an endpoint.

2.2.1 Patient-reported outcomes (PROs)

PROs should be used to measure changes in health and functional status that are of direct relevance to the patient. A PRO should be sensitive to changes in health status. If a multi-dimensional measure is used, it should be clearly stated whether all or some of the dimensions were evaluated. The PRO should encompass domains relevant to the illness being treated. The use of mapping from one PRO to another must be clearly stated and justified. Only a validated mapping function based on empirical data should be used. The statistical properties of the mapping function should be clearly described. All PROs collected in a study should be reported.

Description

The term patient-reported outcome (PRO) covers a whole range of measurement types, but usually refers to self-reported patient health status focussing on how the patient functions or feels in relation to a health condition and its treatment. PROs encompass simple symptom measures (e.g. pain measured by Likert scale), more complex measures (e.g. activities of daily living or function), multidimensional measures (e.g. health-related quality of life) and satisfaction with treatment. PROs

can be generic or disease specific. Generic PROs can be used for any condition but they can be less responsive to changes in health status than disease specific measures. When using a multidimensional PRO, it is important to ensure that the PRO covers all the domains relevant to the illness and technology being assessed, including adverse events. The choice of PRO should therefore be justified based on coverage of relevant domains for the indication of interest.

Health Related Quality of Life (HRQoL) measures can be susceptible to change due to a variety of external factors (e.g. life circumstances unrelated to the illness being treated) with the exception of HRQoL questionnaires that have been specifically developed to capture the impact of a specific disease process. It is possible to map one HRQoL measure onto another, such as EQ-5D onto SF-36. This may be done for comparability to present results using a different HRQoL measure to the one used in a study. Mapping may over- or underestimate the effectiveness of a technology.⁽²²⁾ When mapping from another HRQoL measure, only a validated mapping function based on empirical data should be used. The statistical properties of the mapping function should be clearly described.

Utility measures are used to generate quality adjusted life years (QALYs) which can be used in economic analyses. QALY data are often collected but not reported in a study. QALYs are PROs and provide useful endpoint data for assessing the clinical effectiveness of a technology.

A detailed guidance on the use of HRQoL measures is beyond the scope of this document. References for further reading on HRQoL measures are provided in Appendix 1.

Examples

- EQ-5D™ - a general self-administered questionnaire used to rate health related quality of life across five dimensions (mobility, self-care, usual activities, pain/discomfort, anxiety/depression)⁽²³⁾
- SF-36® - a multi-purpose, short-form health questionnaire consisting of eight scaled scores relating to aspects of physical and mental health⁽²⁴⁾
- WOMAC - a self-administered questionnaire used to evaluate the condition of patients with osteoarthritis of the knee and hip⁽²⁵⁾

Usage

PROs are often used as primary endpoints for technologies that do not have a clear impact on final clinical endpoints but do improve a patient's well-being or functional status. PROs are often collected as secondary endpoints for a trial but may not be reported. Rather than using objective measures of a patient's health status, PROs use subjective self-assessment.

It is possible to detect improvements in a PRO in the absence of a change in a clinical endpoint, and *vice versa*. A technology that improves the PRO but not the primary clinical endpoint can raise questions about the choice of clinical endpoint.

Strengths

PROs measure changes in health and functional status that are of direct relevance to the patient. The use of PROs therefore gives a patient-centred perspective on the effect of a technology. PROs can also highlight where clinicians and patients have divergent views on what endpoints are considered important to patients. A PRO can encompass both the positive and negative effects of a technology in a single summary measure.

PROs can be used to detect endpoints, such as pain, that are difficult or unfeasible to measure in clinical tests.

PROs are often collected in the form of self-administered questionnaires which do not have to be filled out in a clinical setting. There are no requirements for biological samples and they can be assessed by non-clinicians.

Limitations

Some generic PRO measures have been shown to be unresponsive to modest changes in status. If a PRO is not sensitive to change then it may not be able to adequately capture the effect of a technology.

The clinical relevance of PROs can be difficult to determine, except in cases where a PRO is the main efficacy endpoint of the treatment, for example pain used to assess the efficacy of a pain-killer drug.

PRO results are often non-specific to a particular condition and susceptible to general changes in a patient's circumstances making it difficult to associate a change in score directly with the health technology under assessment.

As PROs frequently return a score, it can be difficult to translate the change in score into a marker of clinical improvement. Concepts such as minimal perceptible clinical improvement and responders are used to define clinically significant improvements. The definitions of a clinically significant change and a 'responder' are open to question and must be clearly justified if used.

PROs can be time consuming to complete and if the patient cohort has literacy issues then response rates may be low and the answers may not accurately reflect the true perceptions of the patients.

Critical questions

- Is the PRO a reliable and valid measure of effect?
- Is the PRO sensitive to change?
- Is a change in the PRO clinically significant?
- Is the PRO condition-specific or general?

2.2.2 Clinical endpoints

The choice of clinical endpoint must be justified on the basis of a clear link between the disease process, technology and endpoint.

Description

A clinical endpoint is an aspect of a patient's clinical or health status that is measured to assess the efficacy or harm of a treatment relative to the best available alternative. A clinical endpoint should be a valid measure of clinical benefit due to treatment - it is clinically relevant, sensitive (responsive to change) and is both recognised and used by physicians. Clinical endpoints are based on the presence or absence of measurable clinical events. Clinical endpoints tend to be unambiguous, impartially measured events to minimise potential bias. The choice of clinical endpoint must be justified on the basis of a clear link between the disease process, technology and endpoint.

Examples

- Mortality
- Stroke
- Low birth weight

Usage

Clinical endpoints are perhaps the most common type of endpoints used in clinical trials. They are used in trials where clear clinical events are achieved or avoided due to the treatments being studied. All-cause mortality is considered to be the most unbiased clinical endpoint as it is final and its measurement is unambiguous.

Strengths

Clinical endpoints tend to be both valid and reliable. Clinical endpoints are typically objectively measured, reducing the occurrence of assessment bias. Clinical endpoints are usually generalisable across settings and can therefore improve the external validity of a trial.

Limitations

Clinical endpoints can be poorly defined in studies (e.g. does non-fatal myocardial infarction include silent events?). Differences in definition can lead to quite different results. A clinical endpoint may be a rare event which raises issues of statistical power and the need for large sample sizes. Some clinical endpoints may be clinically important but of little direct relevance to the patient.

Critical questions

- Is the clinical endpoint clearly defined?
- Is there a clear mechanism of action between the technology and the clinical endpoint?
- Is the clinical endpoint objectively or subjectively measured?

2.2.3 Surrogate endpoints

A surrogate endpoint must have a clear biological or medical rationale or have a strong or validated link to a final endpoint of interest. The magnitude of the effect on the surrogate should be similar to that on the final endpoint.

Description

A surrogate endpoint, also called intermediate endpoint, is an objectively measured endpoint that is expected to predict clinical benefit or harm based on epidemiologic, pathophysiologic, therapeutic and other scientific evidence. They are typically physiological or biochemical markers that can be relatively quickly and easily measured. The effect of the technology on the surrogate end point must predict the effect on the clinical endpoint.⁽²⁶⁾ The effect on the surrogate should be of a similar magnitude to the effect on a final endpoint.

If surrogate endpoints are assessed, caution must be exercised in directly extrapolating from these to final endpoints unless underpinned by a clear biological or medical rationale or have a strong or validated link. Although a surrogate endpoint may have a strong link to an endpoint of interest, it may not itself represent a meaningful endpoint to the patient.

Examples

- Blood pressure as a surrogate endpoint for cardiovascular mortality
- Bone mineral density as a surrogate for bone fracture
- HIV1-RNA viral load as an indicator of viral suppression

Usage

Surrogate endpoints are common where final endpoints might require a long follow up period. Surrogate markers are often used when the primary endpoint is either undesired (e.g. death) or when the number of events is very small, thus making it impractical to conduct a clinical trial to gather a statistically significant number of endpoints.

Strengths

When there is a clear and strong link to a final endpoint, a surrogate can enable a shorter follow up period and greatly reduce the cost of a trial.

Limitations

If the mechanism of action of the technology is not fully understood it is possible that the surrogate endpoints will fail to accurately predict the true clinical effect of the technology. Furthermore, if multiple causal pathways exist between the technology and the clinical endpoint then the surrogate may also fail to predict the clinical effect.

The magnitude of the effect on the surrogate may be substantially different to that on the final endpoint. Thus the use of a surrogate may under- or over-estimate the effect of the technology.

Critical questions

- Has a surrogate endpoint been used for convenience?
- Does the surrogate have a clear biological or medical rationale or have a strong or validated link to a final endpoint of interest?
- Can the biomarker be reliably detected?
- Is the magnitude of the effect on the surrogate similar to that on the final endpoint?

2.2.4 Composite endpoints

A change in composite endpoints should be clinically meaningful. All of the individual components of a composite must be reliable and valid endpoints. The components that drive the composite result should be identified.

Description

Composite endpoints combine multiple single events into one endpoint showing the overall and clinically relevant treatment effect. They are often used to increase event rates and decrease sample size where statistical power is poor, and to avoid the issue of multiple testing. Each of the endpoints included in the composite must meet the requirements of validity, reliability, relevance and accurate measurement. The composite may include a mixture of direct clinical and surrogate endpoints. It is important that patients are followed up after the first non-fatal event as they may subsequently experience further events, including a fatal event.⁽²⁷⁾ If non-fatal events are included in a composite endpoint, it is important to state whether all non-fatal events were evaluated or just the first event to occur.

Although trials are often underpowered to report disaggregated endpoints, they should be reported individually where possible.

Examples

- Mortality, hospitalisation and cardiac arrest in chronic heart failure patients
- Mortality, myocardial infarction and stroke in patients with hypertension
- Mortality and new-onset diabetes

Usage

Composite endpoints are most commonly used in studies of cardiovascular technologies. On average composites include three endpoints but can range from two to nine or more.⁽²⁸⁾

Strengths

Composite endpoints can make it possible to estimate the net benefit of a treatment. A composite avoids the problem of selecting a single endpoint where there may be a number of endpoints of equal importance. The use of composites avoids the need to adjust for multiple comparisons.

Limitations

Interpretation of composites can cause problems particularly if the combination consists of endpoints with very different clinical importance or a combination of objective and subjective measures.⁽²⁹⁾ Identifying what could be considered a clinically significant change may be difficult. Interpretation of the results will be complicated if the effect on the composite is driven by the effect on one of the components. Although it may be tempting to conclude that the treatment has a significant impact on the component, it is likely that the data are underpowered to draw such a conclusion.

If the composite endpoint is not given in disaggregated form it may not be viable to combine the results of several studies due to differences in definition (e.g. use of different components). Varying definitions of composite endpoints can lead to substantially different results and conclusions.⁽³⁰⁾

As a composite requires a number of endpoints there is an increased risk of missing data. Inappropriate adjustment for missing data can result in biased estimates of the proportions of successes in composite endpoints.⁽³¹⁾

Critical questions

- Does the composite endpoint really measure treatment effect for a disease?
- Does the use of a composite endpoint solve a medical problem or is it just for statistical convenience?
- Are the individual components of the composite endpoint valid, biologically plausible, and of importance for patients?
- Are the results clear and clinically meaningful? Do they provide a basis for therapeutic decisions? Does each single endpoint support the overall result?
- Is the statistical analysis adequate?

2.2.5 Adverse outcomes

All adverse effects that are of clinical or economic importance must be reported. Both the severity and frequency of harms should be reported. It must be clear whether harms are short-term or of lasting effect.

Description

Many technologies have side-effects which are unintended effects that may be harmful. It is generally anticipated that the benefits of a technology will exceed the potential harms. Endpoints can include adverse events that reflect the safety of a technology. Harms caused by a technology provide an important counterbalance to benefits and can include harm to the patient or to the clinician providing the technology (e.g. radiation exposure during diagnostic imaging). Harms can be broadly classified as effects and reactions. Effects are caused by a technology, while patients experience a reaction.

For many adverse events it may be difficult to definitively ascribe them to a technology. Adverse reactions are often collected as secondary endpoints and there

is likely to be variation across studies in how adverse events are reported in terms of both detail and terminology.

Any differences between the trial population and the intended target population should be reported as disparities may result in a different profile of adverse events between the two populations.

As serious adverse reactions are usually anticipated to be relatively rare, studies are not typically powered to detect differences in their occurrence. To overcome the problem of statistical power, studies often aggregate the adverse outcomes even though they may be of varying importance or severity. Furthermore, relatively minor events (e.g. low grade fever) will not be of much importance in studies where the primary objective is a reduction in mortality. Sufficient follow-up is required to capture important adverse events such as mortality.

Examples

- Hospitalisations due to an adverse drug reaction
- Post-operative complications
- Toxicity-related side effects due to external beam radiotherapy

Usage

Trials of technologies are generally designed to evaluate benefits rather than harms. Trials are generally run over relatively short time horizons with small numbers of patients. Such trials are therefore at most able to detect and quantify frequent adverse events that occur early during treatment. In addition, to be recorded systematically in a trial it must be known beforehand or anticipated that there will be adverse events.⁽³²⁾

Due to the difficulties in making a causal link between a technology and a harm, adverse events are often distinguished from potential adverse events which may have arisen despite the technology. The distinction between preventable and unavoidable adverse events is also used. Preventable events stem from errors such as incorrect drug, dose or frequency.

Strengths

Harms are relevant to patients and may influence whether or not a treatment is acceptable to patients.

Adverse events can have a major impact on cost-effectiveness as they may generate substantial additional treatment costs.

Limitations

Adverse outcomes tend to measure rare events with most studies underpowered to detect statistically significant differences. If a difference in aggregated harms is observed then it may be difficult to conclude whether the difference is due to harms of greater or lesser severity.

Adverse outcomes can be quite different to the endpoints collected to determine treatment effect making it difficult to carry out a direct comparison of benefit and harm.

Adverse events may be recorded by a variety of means (e.g. patient, nurse, doctor) leading to variable quality of reporting and coverage. Furthermore, the outcomes reported may be very different depending on whether they were reported by a clinician or a patient.

Drug therapies often fail due to interactions with concomitant medications taken by a patient. If a study excludes patients with co-morbidities or older patients, then there will be less opportunity for serious drug interactions to arise, even though they may occur frequently in routine practice.

Critical questions

- How have the safety endpoints been collected and reported?
- Are both the severity and frequency of harms quantified?
- Do the harms have lasting effect or are they short-term only?
- Has sufficient follow up been used to capture all important adverse events?

2.2.6 Sensitivity and specificity

The sensitivity and specificity of a diagnostic or screening test should be measured in relation to a recognised reference test. The threshold for a positive test result should be clearly defined.

Description

Sensitivity and specificity are standard measures of diagnostic and screening test accuracy. Although they are not a direct measure of clinical effect, diagnostic tests are used to identify and monitor the existence, onset, severity or risk of disease. As such, they are used as a means to evaluate clinical effects.

Sensitivity and specificity are calculated by comparing the index test to a gold standard test. Sensitivity shows positive index test results as a proportion of those that are genuinely positive based on a gold standard diagnostic or screening test. The specificity indicates negative index test results as a proportion of those that are genuinely negative based on the same gold standard. A perfect test would have a sensitivity and specificity both equal to 100. A test with high sensitivity helps rule out the disease when the result is negative, whereas a test with high specificity helps rule in the disease when the result is positive.

As the calculation of sensitivity and specificity require a dichotomous outcome (i.e. positive or negative), a threshold value must be used to convert continuous or categorical parameters into dichotomous values. Varying the threshold will impact on the sensitivity and specificity of the test.

Different thresholds result in different sensitivities and specificities and the resulting pairs can be illustrated on a receiver operator characteristic (ROC) plot. Sensitivity and specificity are typically negatively correlated so the choice of threshold is a trade-off between high sensitivity at the expense of low specificity or *vice versa*.

Examples

- Magnetic resonance imaging for detection of acute vascular lesions
- Computed tomography in the diagnosis of lymph node metastases in women with cervical cancer
- Electrocardiography for the diagnosis of left ventricular hypertrophy

Usage

Sensitivity and specificity have no clinical value of themselves. However, they are used to calculate other useful characteristics such as the positive and negative likelihood ratios and the diagnostic odds ratio. The likelihood ratios are combined with pre-test odds to calculate the post-test odds of disease. Hence a clinician can determine the probability of presence or absence of disease on foot of a positive or negative test result.

If the sum of sensitivity and specificity is equal to 100, then the test provides no diagnostic evidence.

Strengths

Sensitivity and specificity provide a combined measure of diagnostic test accuracy.

Limitations

Diagnostic test accuracy studies are common but the reporting is often of poor quality and subject to numerous forms of bias.⁽³³⁾ In particular it is vital that those assessing the results of the gold standard test are blinded to the results of the index test. The same gold standard should be applied throughout and the index test should not form part of the gold standard.

Both sensitivity and specificity need to be reported together. Subject to threshold effects, the pre-test odds must be known in order to calculate the post-test probability of a given test result.

Critical questions

- What gold standard is the diagnostic test being compared to?
- Does the test have diagnostic value?
- Should the test be used to rule-in or rule-out disease?
- Does the test make use of a threshold value and how has that been defined?
- How high was the disease prevalence in the study sample?

3 Methods of comparison or meta-analysis

The clinical effectiveness of a technology is generally measured in a randomised controlled trial (RCT) setting. Multiple trials may attempt to estimate the clinical effectiveness of the same technology and will provide different estimates of the effectiveness. Often a single trial may fail to detect a modest but clinically and statistically significant difference between two technologies, mainly due to inadequate numbers of patients. To maximise the evidence base and improve precision it is common to combine results from several trials in a meta-analysis. The process of combining trials involves a weighted average typically related to precision of each trial estimate.

For the purposes of these draft Guidelines it is presumed that sufficient data of acceptable quality are available to justify a meta-analysis. It is assumed that the collected measures of effect comply with the draft Guidelines of section 2. It is also assumed that the collection of the data contributing to the comparisons involves an exhaustive search of published and unpublished trials, and a rigorous selection process based on the methodological quality of the trials.

The purpose of this chapter is to give guidance on appropriate methods of combining measures of effect from multiple trials and to outline some of the common issues associated with those methods. The chapter is structured as follows:

- Section 3.1 discusses common considerations to be taken into account when conducting or assessing a meta-analysis
- Section 3.2 describes networks of evidence
- Section 3.3 provides guidance on how to select the most appropriate method for a given meta-analysis
- Section 3.4 outlines the various methods of meta-analysis

3.1 Common considerations when undertaking or evaluating meta-analysis

There are a number of common important considerations that must be taken into account when undertaking or evaluating the results of a meta-analysis.

3.1.1 Gathering evidence

The methods used to gather evidence for a meta-analysis must be clearly described. Evidence is typically gathered using a systematic review.

Data from trials are used as evidence of treatment effect. In combining data from multiple trials a first step is to identify the relevant studies. The methods used to gather evidence for a meta-analysis must be clearly described. Evidence is typically gathered using a systematic review.

A systematic review of a clinical technology is a review of the evidence regarding that technology prepared using a systematic approach.⁽³⁴⁾ The study question to be

addressed should be defined in advance along with clear inclusion and exclusion criteria. A clear protocol should be prepared outlining the steps of the review. The use of a systematic approach will reduce the likelihood of bias.

The typical steps in a systematic review are as follows⁽³⁵⁾:

- Formulate the review question
- Define inclusion and exclusion criteria
- Identify studies
- Select studies for inclusion
- Assess study quality
- Extract data
- Analyse and present results
- Interpret results

A systematic review may include a meta-analysis of the evidence, but it is not a prerequisite. However, a meta-analysis should preferably be undertaken as part of a systematic review to minimise bias in study selection. In some instances, such as where the researchers have conducted all relevant trials on a particular technology, a systematic review would be unnecessary prior to a meta-analysis. There are a number of texts available that provide clear guidance on how to carry out a systematic review. A list of appropriate guidance texts is provided in Appendix 1.

The pooling of data is sometimes used as an alternative to formal meta-analysis. Data pooling can be used to combine data from across multiple trial locations or when using individual patient data. Data pooling does not require a systematic review.

3.1.2 Individual patient data

Individual patient data can be analysed in a meta-analysis. Individual patient data meta-analysis should not be used to the exclusion of other relevant data. Results should be compared to a study-level analysis.

While meta-analyses typically combine study level effect estimates, it is also possible to combine individual patient data (IPD) from studies. Use of individual data can improve the ability to include comparable subgroups or common endpoints which may not be reported in published studies. Analysis of patient data also enables more detailed time-to-event data to be combined.

The methods of IPD meta-analysis can be broadly classified into two groups: a one-step analysis, in which all patients are analysed simultaneously as though in a mega-trial, but with patients clustered by trial; or a two-step analysis in which the studies are analysed separately, but then summary statistics are combined using standard meta-analysis techniques.⁽³⁶⁾ One-step analysis can be undertaken without clustering at the study level thereby ignoring the distinction between studies, but this is not recommended.

A number of advantages to IPD meta-analysis over aggregate data analysis have been cited including:⁽³⁷⁾

- Original study data are used which gives access to all endpoints recorded
- Consistent inclusion and exclusion criteria and subgroups can be defined across studies
- Potentially longer follow-up data may be available than in published results
- Results of unpublished studies can be included, reducing potential publication bias
- Uniform analytical methods can be applied across all studies
- Better handling of covariates and prognostic factors

By modelling the individual risk across hundreds or thousands of patients, IPD meta-analyses generally have much higher power to detect differences in treatment effect than the equivalent aggregate data analyses that may have fewer than 10 studies.⁽³⁸⁾ An IPD analysis can also be used to determine the potential treatment effect for individual patients rather than at a group level, which may be more relevant to patients. Methods are also available to combine IPD and aggregate data in a single model.⁽³⁹⁾

The main disadvantage of IPD meta-analysis is that data collection is both expensive and time-consuming, and it may not be possible to acquire data from all relevant studies. When the number of patients involved is large, of the order of tens of thousands, the analysis becomes computationally intensive. Using data from a limited number of studies may distort the results and the estimates from an IPD analysis should be compared to the equivalent study-level analysis.

IPD from a single or small number of trials may also be used as a basis for developing a micro-simulation model. Patient characteristics are used to populate the model and simulate the impact of introducing a treatment in terms of endpoints and costs. Such an exercise should not be considered as either evidence synthesis or meta-analysis, but rather a form of sub-group analysis. The use of IPD for micro-simulation is beyond the scope of these draft Guidelines.

3.1.3 Types of study

Evidence to support the effectiveness of a technology should be derived by clearly defined methods. Where available, evidence from high quality RCTs should be used to quantify efficacy.

Studies evaluating the effectiveness of technologies are generally derived from either RCTs or observational studies.

RCTs demonstrate the effect of the technology and randomisation to minimise bias between cases and controls. Patients enrolled in an RCT are often carefully selected, may have few if any co-morbidities and may not be receiving any concurrent treatment, thereby making it difficult to generalise the results. RCTs are often limited

to non-rare diseases and short durations, and ethical considerations can impact on the choice of comparator technology.

Observational studies follow patients in the real world where treatment may be less carefully monitored, and the patients will often have co-morbid conditions for which they are also being treated. Observational studies can have large sample sizes, longer follow-up periods and can therefore be used for rare diseases. They can be based on routinely collected administrative data which greatly reduces the cost of data collection. Due to the numerous possible confounders, it can be difficult to infer a treatment effect in observational studies. They are also open to numerous sources of bias.

For the purposes of a comparison, efficacy should be quantified using high quality RCT data where available. Given the difficulties in assessing bias, observational studies do not always offer the best level of evidence, but they do provide valuable evidence on impact a treatment will have in routine care.

3.1.4 Data and study quality

Studies included in a meta-analysis should be graded for quality of evidence. The quality of evidence should be clearly stated. The results of a meta-analysis should be reported according to relevant standards.

When combining a number of trials it is essential to assess the quality of the data that are to be combined. A meta-analysis of low quality data will not yield a high quality effect estimate.

A trial may be of genuinely poor quality due to inadequate study design, or it may be poorly reported irrespective of the actual study quality. It can be anticipated that a poor quality study will generate a biased estimate of effect.

A poorly reported study may be of good quality but there is insufficient information to safely draw that conclusion. However, a well designed study will typically adhere to good reporting guidelines.

The CONSORT statement was developed to give guidance on best practice for reporting RCTs.⁽⁴⁰⁾ CONSORT includes a 25 item checklist of key characteristics (e.g. trial design, endpoints, technologies, blinding) that must be included in the reporting of an RCT. A trial that reports according to the checklist provides sufficient information to accurately gauge study quality. CONSORT is based on a standard two-group parallel study design but variations are available for other randomised study designs. Standards are also available for the reporting of observational studies (STROBE) and the meta-analysis of observational studies (MOOSE).^(41;42)

While guidelines for reporting strive to improve standards, they do not provide an explicit means of assessing study quality. There are a number of systems available for grading the quality of evidence presented in a study, including GRADE⁽⁴³⁾ and the NHMRC Designation of Levels of Evidence.⁽⁴⁴⁾ The level of evidence is primarily

driven by the study design with an RCT providing the best evidence. The GRADE system can also be applied to studies of diagnostic test accuracy where different considerations apply.⁽⁴⁵⁾

There are also guidelines and standards available for the reporting of meta-analyses. The QUORUM statement lists the key features of a meta-analysis that should be clearly reported (e.g. study selection, data abstraction, heterogeneity assessment).⁽⁴⁶⁾ The QUORUM statement has been revised to encompass advances in systematic reviews and is now known as PRISMA.⁽⁴⁷⁾ An equivalent standard, the QUADAS statement, outlines the critical elements when reporting a meta-analysis of diagnostic test accuracy studies.⁽⁴⁸⁾

3.1.5 Heterogeneity

Heterogeneity of treatment effect between studies must be assessed. Where significant heterogeneity is observed, attempts should be made to identify its causes. Substantial heterogeneity must be dealt with appropriately and may preclude a meta-analysis.

It is assumed that the relative effectiveness of a treatment is the same across all studies included in a meta-analysis – that is, similarity of studies is assumed. If the results of the studies are very different then heterogeneity is observed and combining the results may not be appropriate.⁽⁴⁹⁾ Three broad forms of heterogeneity have been identified: statistical, where effect estimates vary more than expected by chance alone; clinical, due to differences in patient populations or study settings; and methodological, arising from differences in study design and analysis.⁽⁵⁰⁾ These three forms of heterogeneity are not mutually exclusive and will sometimes overlap.

It is possible to test for heterogeneity to provide evidence on whether or not the study results differ greatly or whether or not they are all measuring the same treatment effect. If studies agree on the direction of treatment effect, but disagree on the scale then it may still be possible to draw conclusions from a meta-analysis. However, if significant differences are observed in both the direction and scale of effect then it is unlikely that conclusions can be drawn from a meta-analysis.

Examples of common heterogeneity measures include I^2 and Q statistics although the interpretation of these is subjective. These measures do not provide an optimal way to assess heterogeneity and, where significant heterogeneity is observed, it is critical to closely examine the studies being combined. Such an examination is typically based on a qualitative assessment of the studies in terms of study populations, endpoint measures and other study characteristics.

There can be many causes of heterogeneity such as variations in study design, study subjects, setting, geographic location, and endpoint measures. In some instances it will be possible to partially explain heterogeneity between studies by differences such as those listed above. Even if the variability can be explained, there must still be a decision as to whether or not to proceed with the meta-analysis and whether to

consider subgroup analyses. A subgroup analysis can involve including studies that are considered equivalent according to a more narrowly defined set of criteria (e.g. age range of study participants). It may also be possible to analyse a common subgroup of patients across studies based on a characteristic, for example age group, gender or disease risk.

The presentation of the results of a meta-analysis is frequently accompanied by a forest plot showing the treatment effect estimate of each individual study along with the pooled average. A forest plot provides a relatively simple means of visually assessing heterogeneity and study precision.

3.1.6 Meta-regression

When there is significant between-study heterogeneity, meta-regression is a useful tool for identifying study-level covariates that modify the treatment effect.

The interpretation of the results of a meta-analysis can become complicated when there is significant between-study heterogeneity. While it is possible to allow for the between-study variation by using a random-effects meta-analysis, it can be useful to try and understand the sources of heterogeneity by using a method called meta-regression.

The use of a meta-regression enables incorporation into the meta-analysis of study characteristics that may help explain some of the observed heterogeneity.

Meta-regression would generally be considered as part of a random effects model in that it is understood that covariates are required to explain differences between the studies, whereas a fixed effects meta-analysis presumes equivalence of the studies.

3.1.7 Fixed and random effects

The choice between a fixed and random effects analysis is context specific. Heterogeneity should be assessed using standard methods. Significant heterogeneity suggests the use of a random effects model. Justification must be given for the choice of fixed or random effects model.

In fixed effects meta-analyses, the true effect of treatment is assumed to be the same in each study. Use of a fixed effects model therefore follows from the assumption that variability between studies is entirely due to chance. In a random effects meta-analysis the treatment effect in each study is assumed to vary around an overall average treatment effect.⁽⁵¹⁾ As the random effects model assumes a different underlying effect for each study it tends to result in wider confidence intervals than the fixed effects model.⁽⁴⁹⁾ When the reported effect sizes are homogeneous the fixed and random effects approaches yield very similar results. The choice between random and fixed effects models is context specific and the

decision is often made following an assessment of heterogeneity. Substantial heterogeneity suggests the use of a random effects model but also raises the question of whether the studies are actually comparable, sometimes referred to as comparing apples, oranges and pears. In analyses of sparse event data, as is common for adverse outcomes, it is common to use a fixed effects analysis possibly due to a lack of evidence of heterogeneity. The use of random effects has implications for the interpretation of results and the distribution of effect estimates should be discussed.⁽⁵²⁾

A measure of heterogeneity should be reported to support the choice between a fixed and random effects model. Where heterogeneity is present and a meta-analysis is justified then use of a fixed effects model would not be correct. In such instances a fixed effect model should only be presented in special situations, such as few studies and strongly differing sample sizes.

3.1.8 Sources of bias

Attempts should be made to identify possible sources of bias such as publication bias, sponsorship bias and bias arising from the inclusion of poor quality studies. Potential sources of bias must be reported along with steps taken to minimise the impact of bias.

The issue of publication bias arises due to journals being more likely to publish studies showing beneficial effects of treatments while equally valid studies showing no significant effect remain unpublished.⁽⁵³⁾ The consequence of this bias is that a meta-analysis will show a spurious significant effect. Publication bias may be detectable using funnel plots or regression methods but these are not particularly powerful techniques.⁽⁵⁴⁾ Asymmetry in a funnel plot may indicate publication bias or it may be a reflection of how comprehensive the search strategy has been. The trim and fill technique can be used to adjust for observed publication bias.⁽⁵⁵⁾

English language bias and citation bias are forms of publication bias in which studies with negative findings are more likely to appear in non-English language publications and are less likely to be cited, respectively. It is of critical importance that the search strategy element of the systematic review is as comprehensive as possible and that clinical trial registers are searched, where possible. The presence of publication bias can affect any meta-analysis irrespective of the methodology used (i.e. direct, indirect or mixed treatment comparison).

Bias may also be introduced where some studies are sponsored by the technology manufacturer. In such trials there is a risk that the comparator technology may be applied in a sub-optimal manner to show the active treatment in a more favourable light. Published studies should be examined for stated conflict of interest or study funding that might indicate potential sponsorship bias.

Studies of diagnostic test accuracy are also subject to a variety of biases relating to the patients, the index test and the reference standard.⁽⁵⁶⁾ Spectrum bias, for

example, relates to the observed spectrum of severity for the target condition. The study populations should be representative of the types of people who would normally be subject to the diagnostic test. Disease progression bias arises when the condition of patients changes between application of the index and reference tests. Depending on the type of bias and how it has arisen it will lead to over- or under-estimation of the diagnostic test accuracy. The inclusion of healthy control participants and the differential use of reference standards have both been shown to lead to over-estimation of the diagnostic test accuracy.⁽⁵⁶⁾

3.1.9 Frequentist and Bayesian approaches

Both frequentist and Bayesian approaches are acceptable in meta-analysis. The approach taken must be clearly stated.

There are two broad approaches to statistical inference: frequentist and Bayesian. Frequentists state that data are a repeatable random sample, and that parameters are constant during this repeatable process. Bayesians state that data are observed from the realised sample and that parameters are unknown and described by a probability distribution. In essence, for frequentists the parameters are fixed, whereas for Bayesians the data are fixed.⁽⁵⁷⁾

A Bayesian approach incorporates prior information about the parameters of interest. The prior information is combined with the observed data to generate a posterior distribution for the parameters of interest. Prior information can come from a variety of sources such as previous studies or expert opinion. If there is no useful prior information then non-informative or vague priors are used. In the event of non-informative priors a Bayesian analysis typically generates results that are comparable to those from an equivalent frequentist analysis.

A key distinction between the two approaches is evident from the interpretation of the confidence intervals. In the frequentist approach, a 95% confidence interval means that in repeated samples the confidence interval will include the true parameter value 95% of the time. In the Bayesian approach, the 95% credible interval means that given the realised sample there is a 95% probability that the parameter value is in the interval.

Frequentist methods are common and have been widely implemented and applied. Bayesian methods have gained ground in recent years due to increased computing power and readily available software.

3.1.10 Outliers and influential studies

Influential studies and those that are statistical outliers should be identified and reported. The methods used for identifying outliers must be clearly stated. Studies that are outliers should be characterised to determine if they are comparable to the other included studies.

The results of a meta-analysis may be overly influenced or distorted by one or a small number of studies. Similarly, some studies may be outliers in a statistical sense. Outliers and influential studies are not synonymous: an outlier need not necessarily be influential and an influential study need not be an outlier. A first step is to visually inspect a forest plot to identify any unusual data points or where the pooled estimate appears to be driven by a single or small number of studies.

A variety of techniques are available to identify influential studies and potential outliers. These include metrics such as standardised residuals, Cook's distance, DFFITS and DFBETAS.⁽⁵⁸⁾ Sensitivity analysis techniques based on leave-one-out can be used to determine the impact of influential studies and outliers on the results of a meta-analysis. It is also useful to characterise outliers and gain an understanding of why they might be different from other studies.

3.1.11 Sensitivity analysis

If potential outliers have been identified, or if plausible subgroups of patients or studies have been identified, a comprehensive sensitivity analysis should be conducted. In a Bayesian analysis the choice of priors should be tested using a sensitivity analysis.

The results of a meta-analysis can be sensitive to a variety of factors but the choice of included studies is clearly critical. To test the effects of decisions about which studies to include or exclude, it is advisable to use sensitivity analysis. If potential outliers have been identified then it is pertinent to examine the effect of excluding those studies from the analysis. Similarly it is useful to determine the impact of influential studies on the results.

If plausible subgroups have been identified then it may be possible to carry out a separate meta-analysis for each subgroup. Subgroups can sometimes be identified based on patient characteristics such as age bands or disease risk. Alternatively, subgroups of trials may be identified according to study characteristics (e.g. study quality if measured using a recognised scale, geographic region).

In many cases there may be a limited number of studies available for a meta-analysis. Clearly if there are limited data available then removing studies may not be

feasible and it may not be possible to carry out a full or comprehensive sensitivity analysis.

In a Bayesian analysis there are decisions relating to the choice of priors which may be informative or non-informative. Where there are informative priors it is important to test how the results compare to those using non-informative priors. However, in the case of non-informative priors a variety of distributions are often available and the choice of distribution may impact on results.

3.2 Networks of evidence

The network of available evidence should be described and used to guide the selection of the method of meta-analysis. The selection of direct and indirect evidence must be clearly defined. The exclusion of relevant evidence, either direct or indirect, should be highlighted and justified. Where direct and indirect evidence are combined, inconsistencies between the direct and indirect evidence must be assessed and reported.

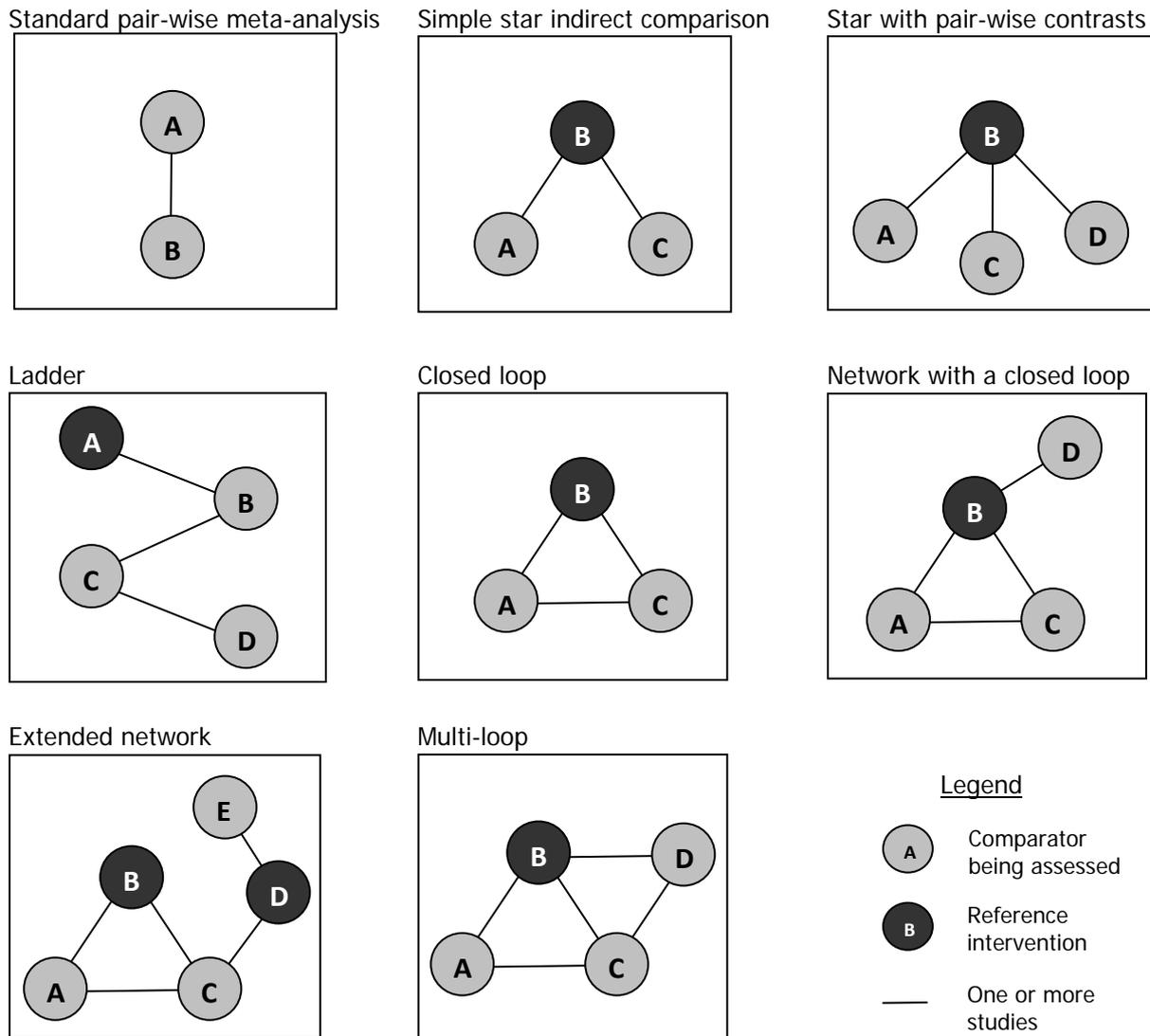
The studies available for a meta-analysis form a network of evidence. The most common comparison is between two technologies based on a number of head to head trials. Such a comparison is called a direct comparison. In cases where two treatments are compared, there is sometimes insufficient data available to reliably estimate the relative effectiveness of the two treatments in which case it may be possible to estimate the relative effectiveness using an indirect comparison.⁽⁵⁹⁾

When there are no head to head trials, but two technologies can be compared based on a common comparator, it is possible to use indirect methods of meta-analysis. There are also approaches that allow direct and indirect evidence to be combined.

Depending on the method of comparison used, there may be restrictions on the type of networks that can be analysed. For direct comparisons only a standard pair-wise meta-analysis can be used.

Alternative networks can include: a star pattern in which two or more treatments have a common comparator (e.g. A-B, C-B, D-B); a ladder where treatment comparisons appear in a sequence (e.g. A-B, B-C, C-D); a closed loop in which there is both direct and indirect evidence (e.g. A-B, A-C, C-B); or a complex combination of patterns such as a closed loop with a star (see Figure 1 on the next page for examples).⁽⁶⁰⁾

Figure 1. Networks of evidence



Direct comparisons involve a meta-analysis combining the results of multiple trials that all compare the treatment of interest to the same comparator (e.g. placebo). Standard meta-analytic techniques are applied for direct comparisons. The primary decision in direct comparisons relates to the choice between fixed and random effects meta-analysis.

Approaches to direct comparisons meta-analysis can be sub-divided into two methodologies: frequentist and Bayesian. The former are standard for direct comparisons primarily due to the ease of application and the variety of software packages available to apply them.

The need for indirect comparisons arises when treatments A and B are being compared but only studies comparing A to C and B to C are available. By using a common comparator, in this case treatment C, an indirect comparison of treatments A and B can be carried out.

Placebo-controlled trials are commonly conducted in preference to head-to-head trials giving rise to the need for indirect comparisons when comparing two active treatments.⁽⁶⁰⁾ Depending on the amount of evidence available, indirect comparisons can sometimes make comparisons via two or more different paths. In comparing treatments A and B, the relative effectiveness should be similar whether derived via common comparator C or D. A statistically significant difference in the estimate of effectiveness would indicate inconsistency.

A multiple treatment comparison combines direct and indirect evidence to compare a technology to two or more other treatments. As a combination of direct and indirect evidence is used, these methods generally provide a measure of inconsistency or incoherence between the direct and indirect evidence. In mixed treatment comparisons, consistency between direct and indirect evidence is assumed. That is, if direct evidence suggests that treatment A is better than treatment B, then that evidence should not be contradicted by the indirect evidence.

In a multiple treatment comparison involving both direct and indirect evidence, the evidence network can become very complex with many comparisons based on only one or two studies. With increasing complexity and greater numbers of treatments, the prospect of inconsistency increases. There is also a power trade-off between the number of pair-wise comparisons and the number of studies included in the analysis – too many comparisons with too few studies and the analysis may be underpowered to detect true differences.⁽⁶¹⁾

3.3 Selecting the method of comparison

The choice of method of comparison depends on the quality, quantity and consistency of direct and indirect evidence. The available evidence must be clearly described along with a justification for the choice of method.

When undertaking a meta-analysis a decision must be made regarding which method of comparison to use. A first question is to ask whether or not there is sufficient evidence to warrant combining data. There are no hard and fast rules to define 'sufficient evidence' in this context but it is based on an evaluation of the quality, quantity and agreement of evidence. Substantial heterogeneity highlights where trials may not be measuring the same effect or where there may be systematic effect moderators.

The choice of method of comparison must take into account the network of evidence and the number of technologies being compared. When there is only direct evidence then the only questions relate to whether the studies should be combined and, if so, whether to use a frequentist or Bayesian approach.

When indirect evidence is available then one must evaluate whether or not to include it and, if so, by which method. An important aspect in evaluating indirect evidence is whether or not it is in agreement with direct evidence. Disagreement between direct and indirect evidence must be fully investigated and it may preclude pooling data if the disagreement cannot be adequately explained. Certain networks of evidence limit

the number of methods available but the researcher will often have some discretion as to how many comparisons to incorporate.

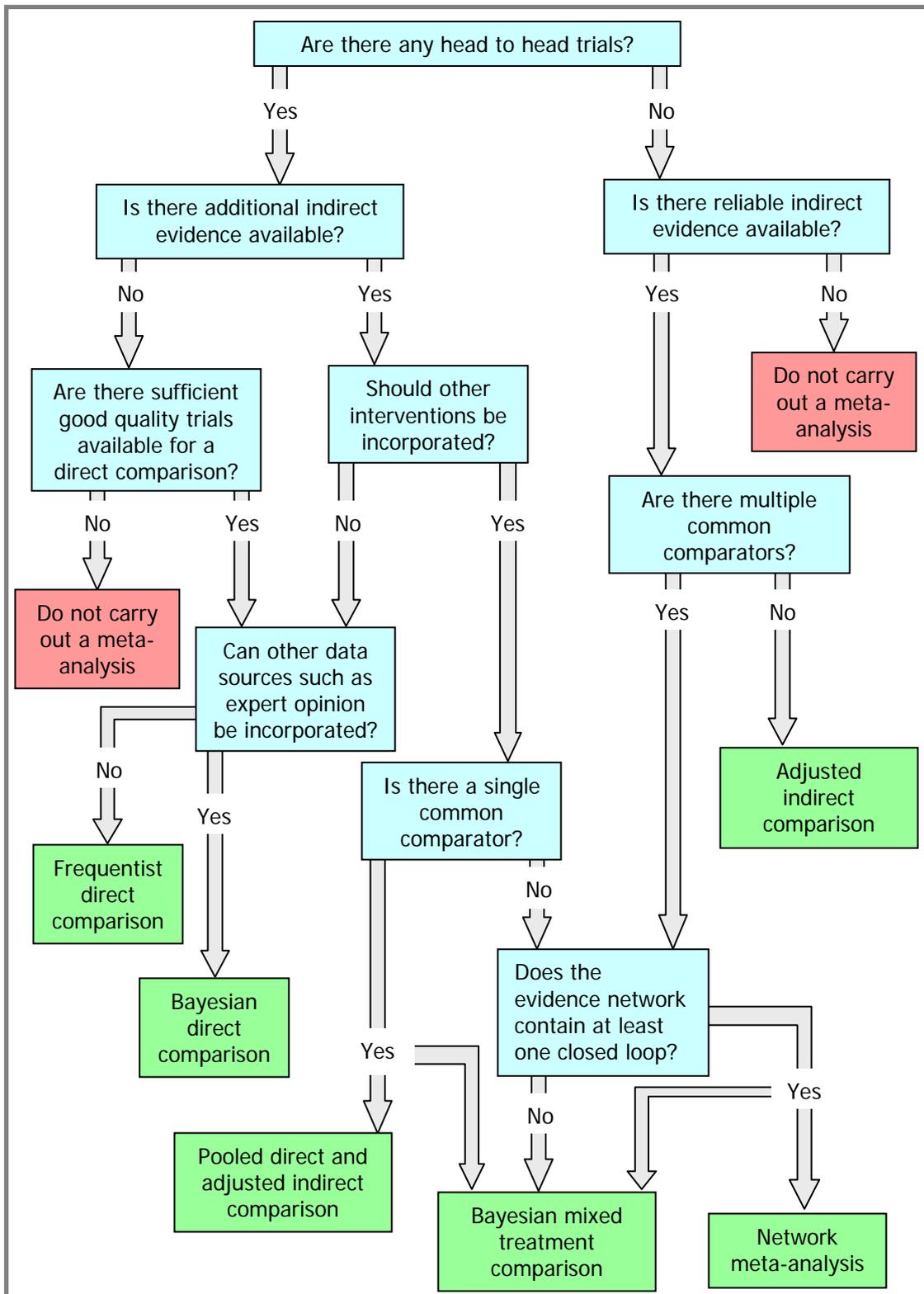
The network can be restricted to include the minimum number of comparisons required to enable an indirect comparison between the technologies of interest. Alternatively it can be expanded to include as many relevant comparators as possible.

Some questions that will assist in selecting the appropriate method of comparison include:

- Are there sufficient head to head trials for a direct comparison?
- Is there reliable indirect evidence available?
- If direct evidence has been excluded - why?
- If indirect evidence has been excluded - why?
- If indirect evidence is used, were all or a subset of available indirect comparisons used?
- For the indirect evidence, is there a single or multiple common comparators?

The process for selecting of the most appropriate method for comparing technologies is shown as a flow diagram in Figure 2 on the next page. If more than one method is available in a given context, then the choice of method should be justified with consideration being given to the possible impact of that choice on the outputs of the analysis.

Figure 2. Selecting the most appropriate method of meta-analysis when comparing technologies



3.4 Methods of meta-analysis

For any method of meta-analysis all included trials must be sufficiently comparable and measuring the same treatment effect.

A variety of meta-analysis methods are available depending on the type of evidence network being analysed. In any method of meta-analysis it is assumed that the relative effectiveness of a technology is the same across all trials used in the comparison. The assumption of constant efficacy requires all trials included in the analysis to be equivalent and attempting to measure the same treatment effect – that is, the results of one set of trials (A vs. B) should be generalisable to the other set of trials (A vs. C). Determining whether the assumption of generalisability holds is a subjective assessment based on a detailed review of the included studies in both comparisons. Were the sets of studies treating the same indications in comparable populations, and were they applying the common treatment in the same manner (e.g. dosing and frequency)?

This section looks at different methods of meta-analysis. For each method of meta-analysis there is a brief description, some examples of published meta-analyses using that method, a brief note on usage in the literature, the strengths and limitations of the methodology and then some critical questions that should be asked when considering that method of meta-analysis.

3.4.1 Direct meta-analysis

Direct meta-analysis should be used when there are sufficient comparable head to head studies available. If indirect evidence is available then consideration should be given to a multiple treatment comparison.

Description

Direct meta-analysis is used for combining head-to-head trials. The methods available for direct comparison meta-analysis are divided into fixed and random effects methods. The confidence intervals around the pooled random effects estimate tend to be wider than would be observed in the fixed effects meta-analysis.

Bayesian methods for direct comparisons meta-analysis are analogous to frequentist methods with the primary distinction being the use of prior distributions for the mean of the overall estimate, the means of the individual estimates of each study, and the between-study variance (for random effects models).⁽⁶²⁾ The use of non-informative priors will generally result in effect estimates that are comparable to those in a frequentist approach.

However, in some instances it may be appropriate to form informative priors by way of other data, such as expert opinion, which are likely to generate results that may differ to those from a frequentist approach.

For certain endpoints, such as rate ratios, a study with zero cases can be problematic. The common solution to this problem is to apply a continuity correction by adding a constant (typically 0.5) to the number of cases. The use of a continuity correction can impact on the significance and interpretation of results.⁽⁶³⁾

Examples

- The safety and efficacy of carotid endarterectomy versus carotid artery stenting in the treatment of carotid artery stenosis⁽⁶⁴⁾
- Aprotinin compared to tranexamic acid in cardiac surgery⁽⁶⁵⁾
- The impact of omega-3 fatty acids on mortality and restenosis in high risk cardiovascular patients⁽⁶⁶⁾
- Adjunctive thrombectomy for acute myocardial infarction⁽⁶⁷⁾
- Double versus single stenting for coronary bifurcation lesions⁽⁶⁸⁾

Usage

The application of Bayesian methods for direct meta-analysis is uncommon primarily because of the greater complexity in computing the models, and the fact that the results tend to be quite similar to those obtained using standard frequentist methods.

Strengths

The methods for direct meta-analysis are well described and can be implemented in a wide variety of software packages. Analyses can be easily reproduced if the underlying data are available.

The strength of Bayesian approaches in this context is that they can incorporate data from a wide variety of sources and can, for example, use expert opinion to elicit useful information. Rather than computing confidence intervals, a Bayesian meta-analysis computes a credible interval which has a different interpretation. A Bayesian approach allows the computation of the probability that one treatment is better than another, which is useful to decision makers.

Limitations

Direct meta-analysis requires head to head trials to compare two technologies. For some treatments it is becoming increasingly difficult to obtain sufficient studies to enable a direct comparison.

A common criticism of Bayesian techniques rests on the use of priors for key parameters. Critics of the Bayesian approach suggest that priors are subjectively chosen. In reality, most Bayesian analyses employ vague or non-informative priors. However, even with a non-informative prior, assumptions are made about the distribution of that prior and often there are alternative formulations available so the use of sensitivity analysis is important.⁽⁶⁹⁾

Critical questions

- Are the studies comparable?
- Has heterogeneity been assessed?
- Was the choice between fixed and random effects clearly justified?
- In a Bayesian analysis, how were the priors defined and were alternatives tested?

3.4.2 Unadjusted indirect comparison

The method of unadjusted indirect comparisons should not be used.

Description

Unadjusted indirect comparisons combine study data as though they had come from a single large trial.⁽⁵⁹⁾ A weighted summary effect is computed for all study arms involving treatment A and is compared to a weighted summary effect for all study arms including treatment B. The relative effectiveness of treatment A is compared to treatment B using the two summary effects. This method is called an “unadjusted indirect comparison” because the indirect comparison does not adjust for events in the control group.⁽⁷⁰⁾

Examples

- Rectal corticosteroids versus alternative treatments in ulcerative colitis⁽⁷¹⁾
- Effectiveness of anticoagulant or platelet anti-aggregant treatment for stroke prevention in patients at elevated risk for stroke⁽⁷²⁾
- The effects of non-steroidal anti-inflammatory drugs on blood pressure⁽⁷³⁾

Usage

The application of unadjusted indirect comparisons is very unusual.⁽⁷⁴⁾ Given the shortcomings of the method it would be difficult to publish an analysis using this methodology.

Strengths

Unadjusted indirect methods provide a simple and easily implemented method of calculating relative effectiveness in the absence of head to head evidence.

Limitations

The primary flaw of this approach is that it ignores the randomised nature of individual trials. When compared to direct estimates, unadjusted direct comparisons result in a large number of discrepancies in the significance and direction of relative effectiveness.⁽⁷⁴⁾ Although unbiased, this method yields unpredictable results and is flawed by not acknowledging randomisation. As such this method of indirect comparison should not be used.

Critical questions

- Why was an unadjusted indirect comparison used rather than an adjusted indirect method?
- How would the results have differed if an adjusted indirect comparison had been applied?

3.4.3 Adjusted indirect comparison

Adjusted indirect comparison is appropriate for comparing two technologies using a common comparator.

Description

Bucher *et al.* presented an adjusted indirect method of treatment comparison that can estimate relative treatment effects for star pattern networks.⁽⁷⁵⁾ This method is based on the odds ratio as the measure of treatment effect, although it can be trivially extended for other measures.⁽⁶⁰⁾ This method is intended for situations where there is no direct evidence (e.g. comparing treatments A and B, but the only evidence is through comparison with C). Certain more complex networks including closed loops can be analysed, but only in the form of pair-wise comparisons.

As the method assumes independence between the pair-wise comparisons, it cannot readily be applied to multi-arm trials where this assumption fails. In a multi-armed trial it is expected that the treatment effect will be correlated between arms.

Examples

- Effectiveness of gemcitabine based combinations compared to single agent gemcitabine in patients with advanced pancreatic cancer⁽⁷⁶⁾
- Effectiveness of nifedipine compared with atosiban for tocolysis in preterm labour⁽⁷⁷⁾
- Comparison of pravastatin, simvastatin, and atorvastatin for cardiovascular disease prevention⁽⁷⁸⁾

Usage

Although initially popular, Bucher's adjusted indirect comparison method is gradually being replaced by other methods, particularly Bayesian mixed treatment comparisons.^(79;80)

Strengths

This method is relatively simple to implement and superior to an unadjusted indirect comparison. It is possible to combine pooled estimates of direct and indirect evidence using inverse variance weights as in a standard meta-analysis.⁽⁸¹⁾

Limitations

This method is applied in the absence of any direct evidence and can only be used in more complex networks of evidence in the form of pair-wise comparisons.

This method is not appropriate when using data derived from multi-arm trials.

Critical questions

- Does the analysis include or exclude multi-arm trials?
- Is direct evidence available that could be incorporated into a network meta-analysis or Bayesian mixed treatment comparison?

3.4.4 Network meta-analysis

A network meta-analysis is appropriate for analysing a combination of direct and indirect evidence where there is at least one closed loop of evidence connecting the two technologies of interest.

Description

The method of network meta-analysis proposed by Lumley allows the combination of both direct and indirect evidence.⁽⁸²⁾ This methodology requires the data to contain a closed loop structure. Depending on the complexity of the closed loop design, it is generally possible to compute relative effectiveness by a number of routes. It is possible to compute the amount of agreement between the results obtained when different linking treatments are used. This agreement forms the basis of an incoherence measure which is used to estimate the consistency of the network paths. Incoherence is used to compute the 95% confidence interval for the indirect comparison. It is assumed that the comparison between two treatments will occur through a closed loop. The measure of incoherence, which is an integral part of the calculation, requires a closed loop.

Examples

- Effects of glucosamine, chondroitin, or placebo in patients with osteoarthritis of hip or knee⁽⁸³⁾
- Efficacy and tolerability of second-generation antidepressants in social anxiety disorder⁽⁸⁴⁾
- Comparison of common antiplatelet regimens after transient ischaemic attack or stroke⁽⁸⁵⁾

Usage

This method can be implemented in a variety of software packages and can be estimated using both frequentist and Bayesian approaches. Network meta-analysis gained popularity over the last decade although in recent years it has been overtaken by Bayesian mixed treatment comparison.

Strengths

Network meta-analysis generates an adjusted indirect treatment comparison that partially preserves the randomisation of study groups in the included trials.

This method simultaneously combines direct and indirect evidence and provides an estimate of the agreement between different results. Direct evidence is not required for this methodology.

Limitations

A network meta-analysis cannot be applied to star- or ladder-shaped networks of evidence.

Network meta-analysis also does not automatically account for correlations that may exist between different effect estimates when they are obtained from a single multi-

armed trial. In trials with more than two treatment arms, estimates of relative treatment effects will be correlated due to the structure of the network of evidence. For example, in a multi-arm placebo-controlled trial the comparison between any two treatments will be correlated with the comparison of each of those treatments with placebo. Accounting for this correlation is possible using a random effects model but this is not considered an optimal solution.⁽⁶⁰⁾

Critical questions

- Does the analysis include multi-arm trials?
- Were multiple paths available to compare two treatments and were they all used to test the consistency of results?

3.4.5 Bayesian mixed treatment comparison

A Bayesian mixed treatment comparison is appropriate for comparing multiple treatments using both direct and indirect evidence.

Description

Bayesian mixed treatment comparison (MTC) meta-analysis is a generalisation of standard pair-wise meta-analysis for A vs. B trials to more complex networks of evidence.⁽⁸⁶⁾ Any combination of studies can be combined as long as every study is connected to at least one other study. Both direct and indirect evidence can be combined and there is no restriction to the number of arms in any given trial. Bayesian MTC facilitates simultaneous inference about all of the treatments included in the analysis, allowing estimation of effect estimates for all pair-wise comparisons and for treatments to be ranked according to relative effectiveness. Bayesian MTC can incorporate meta-regression enabling the addition of study-level covariates as a means to reduce inconsistency although this adaptation has implications for compromised power.^(61;87)

Being a Bayesian approach, there is scope for defining informative priors. While priors may be legitimately generated, it is critical that they are credible and clearly justified.

Examples

- The relative efficacy of existing treatments and combinations to reduce the risk for COPD exacerbations⁽⁸⁸⁾
- The efficacy and safety of bronchodilators and steroids, alone or combined, for the acute management of bronchiolitis in children under 2 years⁽⁸⁹⁾
- The effectiveness of psychological interventions compared to usual care in coronary heart disease⁽⁹⁰⁾

Usage

Bayesian MTC is becoming increasingly popular due to its versatility and the lack of direct evidence. Although forest plots are often presented, the studies have to be grouped by comparator given that multiple comparisons are shown.

Strengths

As this method can be applied to very complex networks it allows more evidence to be incorporated into an analysis. All of the technologies included can be ranked according to the probability that they are the best treatment.

This method pools the effect estimates across trials rather than individual treatment groups. Multi-arm trials can be included and the correlations between arms are taken into account.

Limitations

Bayesian MTC is a complex technique that does not lend itself to easy application or interpretation. The Bayesian framework requires an in-depth understanding, particularly with regard to model checking and the definition of priors.

The key strength that more evidence can be utilised can also represent a weakness as it may be difficult to define limits for the network of evidence, particularly for an indication that has a wide range of treatment options available. Also, in a complex network there may be very little evidence for many of the comparisons.

Critical questions

- How was it decided which comparisons should be included?
- What model was used and is it appropriate for the data?
- How were the priors defined?
- Is there evidence of inconsistency and, if so, has it been explained?

3.4.6 Meta-analysis of diagnostic test accuracy studies

The bivariate random effects and hierarchical summary receiver operating characteristic models (HSROC) should be used for pooling sensitivity and specificity from diagnostic and screening test accuracy studies. The correlation between sensitivity and specificity should be reported.

Description

Diagnostic accuracy studies measure the level of agreement between the results of the test under evaluation and that of the reference standard. The primary endpoint is binary (i.e., a positive or negative test result) and is recorded for both the diagnostic test being assessed and the reference standard. Diagnostic test accuracy is most often represented by sensitivity and specificity.

Meta-analysis of diagnostic test accuracy studies have traditionally used the summary Receiver Operating Characteristic (sROC) curve approach whereby sensitivity and specificity are converted into a single measure called the diagnostic odds ratio.⁽⁹¹⁾ Pooled estimates of sensitivity and specificity can be derived from the sROC curve. However, such an approach ignores the fact that sensitivity and specificity are often correlated.

Bivariate random effects models have come to the fore more recently and take into account any observed correlation between sensitivity and specificity.⁽⁹²⁾ Another method, the hierarchical SROC (HSROC), generates equivalent results in the absence of covariates. By analysing these two parameters and producing a summary estimate of each, it is possible to determine whether the test is better for ruling in or ruling out a particular diagnosis.

Likelihood ratios can also be computed which are of more use to clinicians as they quantify the extent to which a test result changes the probability of disease. If there is no correlation between sensitivity and specificity then it may be more appropriate to carry out separate univariate analyses to pool sensitivities and specificities.⁽⁹³⁾

Examples

- Diagnostic accuracy of natriuretic peptides and ECG in the diagnosis of left ventricular systolic dysfunction⁽⁹⁴⁾
- Diagnostic accuracy of rectal bleeding in combination with other symptoms, signs and tests in relation to colorectal cancer⁽⁹⁵⁾
- Diagnostic accuracy of FDG PET for the characterization of adrenal masses⁽⁹⁶⁾

Usage

The Moses-Littenberg approach is still common for pooling diagnostic test accuracy studies. The bivariate random effects and HSROC methods have gained popularity. It is recommended that the bivariate random effects and HSROC models should be used although they often generate similar results to the traditional techniques.^(97;98)

Strengths

By combining the results of several diagnostic test accuracy studies, it is possible to determine the typical performance of the test. If the threshold for a positive test can be varied, it is possible to determine the performance under different thresholds.

The bivariate random effects and HSROC model the distribution of pairs of sensitivity and specificity from each study. These models give valid estimates of the average sensitivity and specificity and can be extended to include covariates that may explain between-study heterogeneity.

Limitations

The reference standard test itself may not be an accurate measure of disease. This can arise due to a poor choice or because of inconsistent application of the reference standard.

Test accuracy can vary between patient subgroups, disease spectrum, clinical setting, or with the test interpreters and may depend on the results of previous testing.⁽⁵⁶⁾ Failure to ensure that the included studies are fully equivalent will lead to a biased estimate of test accuracy. The Moses-Liitenberg approach fails to consider the precision of the study estimates and does not estimate between-study heterogeneity.

Critical questions

- Do the various studies being pooled use the same threshold for a positive test result?
- Do all the studies use the same reference standard?
- Has the correlation between sensitivity and specificity been reported?

3.4.7 Generalised linear mixed models

Generalised linear mixed models are appropriate when analysing individual patient data from trials.

Description

Regression techniques can be used to combine trial data to evaluate relative effectiveness. Where the primary endpoint is binary and data are available in the form of 2×2 frequency tables for each study, logistic regression can be used. Generalised linear mixed models (GLMMs) have also been proposed as a method of combining trial data in a regression framework.⁽⁷⁴⁾ The application of GLMMs to continuous or time-to-event endpoints requires individual level patient data. The advantage of regression techniques is the potential for including study level covariates that may be used to explain heterogeneity in the measured effects. Although not restricted to meta-analysis of individual patient data, that is the application where GLMMs offer the greatest advantage over other techniques.

GLMMs can offer benefits for meta-analysis as they do not have to be restricted to the within-study normal distribution assumption. They can be extended to other distributions which may be more appropriate particularly for rare event data.⁽⁹⁹⁾

Examples

- Comparison of low-molecular-weight heparin to unfractionated heparin for the treatment of pulmonary embolism and deep vein thrombosis⁽¹⁰⁰⁾

Usage

The application of GLMMs to meta-analysis is relatively rare as less complex techniques for direct meta-analysis and meta-regression are sufficient in most applications. Given the difficulties in obtaining individual patient data, it is unlikely that the advantages of GLMMs will be realised.

Strengths

GLMMs can be applied through most of the leading statistical software packages. It is possible to use exact rather than approximate likelihood approaches by using GLMMS.⁽⁹⁹⁾ They are versatile and can be applied to network meta-analysis (see Section 3.2.5).

Limitations

Individual level patient data can be very difficult if not impossible to obtain.

In many cases GLMMs may not offer substantial advantages over other methods that can be applied more easily.

Critical questions

- If individual patient data are used, have data from an adequate number of studies been included?

3.4.8 Confidence profile method

The confidence profile method can be used to combine direct and indirect evidence. Network meta-analysis or Bayesian mixed treatment comparison should be used in preference to the confidence profile method. The use of this method over other available methods should be justified.

Description

The confidence profile method provides a general framework for undertaking multi-parameter meta-analysis.⁽¹⁰¹⁾ As well as incorporating trials with different treatment comparisons, it can encompass different designs, outcomes and measures of effect. The confidence profile method also allows explicit modelling of biases. Although this method is typically implemented as a fully Bayesian model, it can be formulated without prior distributions and fitted using maximum likelihood methods.⁽⁷⁴⁾ Where there is direct and indirect evidence available, cross-validators predictive checking can be used to determine evidence consistency.⁽⁶⁰⁾ If different doses of the same drug treatment were studied, looking at dose-response relationships can also provide cross-validators information, provided the trial populations are comparable. The models available for this methodology are based on fixed-treatment effects although both fixed and random study-effects are possible.

Examples

- The efficacy of the ketogenic diet in reducing seizure frequency for children with refractory epilepsy⁽¹⁰²⁾
- The efficacy of antibiotics for patients undergoing tube thoracostomy⁽¹⁰³⁾
- The efficacy and complications of cervical spine manipulation and mobilization for the treatment of neck pain and headache⁽¹⁰⁴⁾

Usage

The confidence profile method of meta-analysis never entered common usage and has been replaced by other methods of indirect and multiple treatment comparison.⁽¹⁰¹⁾

Strengths

This method preserves the randomised nature of RCT data. The appropriateness of combining direct and indirect evidence can be assessed using various model-checking statistics.

Limitations

The models for the confidence profile method are relatively complex which has partly restricted their diffusion into general use. Although improvements in computing power and software now make these models more feasible, other methodological developments have come to the fore (e.g. Bayesian mixed treatment comparison).

When there is no direct evidence the cross-validators predictive checking cannot be carried out to determine whether or not the selected studies can be validly combined in an indirect comparison.

Critical questions

- Does the analysis combine direct and indirect evidence?
- Has cross-validators predictive checking been carried out?
- If there is direct evidence is it consistent with the indirect evidence?

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5 Glossary of terms and abbreviations

Some of the terms in this glossary will not be found within the body of these Draft Guidelines. They have been included here to make the glossary a more complete resource for users.

Adverse event: an undesirable effect of a health technology.

Bayesian: a form of statistical inference in which data are observed from a realised sample and the underlying parameters (e.g. mean) are unknown and described by probability distributions. Prior knowledge about the parameters is updated using observed data to generate posterior distributions for the unknown parameters (See also **Frequentist**).

Bias: systematic (as opposed to random) deviation of the results of a study from the 'true' results.

Biomarker: a substance used as an indicator of a response to a therapeutic intervention. An example of a biomarker is the presence of an antibody that may indicate infection.

Comorbidity: the coexistence of a disease, or more than one disease, in a person in addition to the disease being studied or treated.

Comparator: the alternative against which the intervention is compared.

Confidence interval: the computed interval with a specified probability (by convention, 95%) that the true value of a variable such as mean, proportion, or rate is contained within the interval.

Cost-effectiveness: a comparison of both the costs and health effects of a technology to assess whether the technology provides value for money.

Covariate: a variable that may be predictive of the endpoint being analysed. Covariates can be specified for individual patients (e.g. age, sex, disease risk) and for studies (e.g. mean patient age, proportion males).

Critical appraisal: a strict process to assess the validity, results and relevance of evidence.

Direct comparison: a meta-analysis combining multiple head-to-head trials comparing the technology of interest to the same comparator (See also **Indirect comparison** and **Multiple treatment comparison**).

Effectiveness: the extent to which a technology produces an overall health benefit (taking into account adverse and beneficial effects) in routine clinical practice (contrast with **Efficacy**).

Efficacy: the extent to which a technology produces an overall health benefit (taking into account adverse and beneficial effects) when studied under controlled research conditions (contrast with **Effectiveness**).

Final outcome: a health outcome that is directly related to the length of life, e.g. life-years gained or quality-adjusted life years.

Fixed effects analysis: the true effect of the treatment is assumed to be the same in each study and that the variability between studies is entirely due to chance (See also **Random effects analysis**).

Frequentist: a form of statistical inference in which data are considered a repeatable random sample whereas the underlying parameters (e.g. mean) are fixed. If a trial is repeated enough times the sample mean will approach the true mean (See also **Bayesian**).

Generalisability: the problem of whether one can apply or extrapolate results obtained in one setting or population to another. Term may also be referred to as 'transferability', 'transportability', 'external validity', 'relevance', or 'applicability'.

Health outcome: a change (or lack of change) in health status caused by a therapy or factor when compared with a previously documented health status using disease-specific measures, general quality of life measures or utility measures.

Health technology: the application of scientific or other organised knowledge – including any tool, technique, product, process, method, organisation or system – in healthcare and prevention. In healthcare, technology includes drugs, diagnostics, indicators and reagents, devices, equipment, and supplies, medical and surgical procedures, support systems and organisational and managerial systems used in prevention, screening diagnosis, treatment and rehabilitation.

Health technology assessment (HTA): this is a multidisciplinary process that summarises information about the medical, social, economic and ethical issues related to the use of a health technology in a systematic, transparent, unbiased, and robust manner. Its aim is to inform the formulation of safe, effective health policies that are patient focused and seek to achieve best value.

Heterogeneity: in the context of meta-analysis, clinical heterogeneity means dissimilarity between studies. It can be because of the use of different statistical methods (statistical heterogeneity), or evaluation of people with different characteristics, treatments or outcomes (clinical heterogeneity). Heterogeneity may render pooling of data in meta-analysis unreliable or inappropriate. Finding no significant evidence of heterogeneity is not the same as finding evidence of no heterogeneity. If there are a small number of studies, heterogeneity may affect results but not be statistically significant.

Incidence: the number of new cases of a disease or condition that develop within a specific timeframe in a defined population at risk. It is usually expressed as a ratio of the number of affected people to the total population.

Indication: a clinical symptom or circumstance indicating that the use of a particular intervention would be appropriate.

Indirect comparison: a meta-analysis in which the technology of interest is compared to the comparator technology via a third technology. This method is used in the absence of any head-to-head trials comparing the technology of interest to the comparator technology (See also **Direct comparison** and **Multiple treatment comparison**).

Meta-analysis: systematic methods that use statistical techniques for combining results from different studies to obtain a quantitative estimate of the overall effect of a particular intervention or variable on a defined outcome. This combination may produce a stronger conclusion than can be provided by any individual study. (Also known as data synthesis or quantitative overview).

Multiple treatment comparison: a meta-analysis using a combination of direct and indirect evidence to determine the relative effectiveness of three or more technologies (See also **Direct comparison** and **Indirect comparison**).

Multi-arm trial: a trial evaluating more than two treatments with a patient group for each treatment.

Outcome: consequence of condition or intervention; in Economic Guidelines, outcomes most often refer to health outcomes, such as surrogate outcomes or patient outcomes.

Prevalence: the number of people in a population with a specific disease or condition at a given time and is usually expressed as a ratio of the number of affected people to the total population.

Probability: expression of degree of certainty that an event will occur, on scale from zero (certainty that event will not occur) to one (certainty that event will occur).

Probability distribution: portrays the relative likelihood that a range of values is the true value of a treatment effect. This distribution often appears in the form of a bell-shaped curve. An estimate of the most likely true value of the treatment effect is the value at the highest point of the distribution. The area under the curve between any two points along the range gives the probability that the true value of the treatment effect lies between those two points. Thus, a probability distribution can be used to determine an interval that has a designated probability (e.g. 95%) of including the true value of the treatment effect.

Quality-adjusted life year (QALY): a unit of healthcare outcomes that adjusts gains (or losses) in years of life subsequent to a healthcare intervention by the quality of life during those years. QALYs can provide a common unit for comparing cost-utility across different technologies and health problems. Analogous units include Disability-Adjusted Life Years (DALYs) and Healthy-Years Equivalents (HYEs).

Random effects analysis: the treatment effects in each study is assumed to vary around an overall average treatment effect. A random effects analysis therefore

assumes a different underlying effect for each study (See also **Fixed effects analysis**).

Receiver operating characteristic (ROC) curve: a graphical plot of the true positive rate against the false positive rate. The ROC curve is used as a fundamental tool for evaluating diagnostic tests.

Reliability: the extent to which repeated measures of the same endpoint return the same value (See also **Validity**).

Sensitivity analysis: a means to determine the robustness of a mathematical model or analysis by examining the extent to which results are affected by changes in methods, parameters or assumptions.

Statistical significance: a conclusion that a technology has a true effect, based upon observed differences in outcomes between the treatment and control groups that are sufficiently large so that these differences are unlikely to have occurred due to chance, as determined by a statistical test. Statistical significance indicates the probability that the observed difference was due to chance if the null hypothesis is true. It does not provide information about the magnitude of a treatment effect. (Statistical significance is necessary but not sufficient for clinical significance.)

Stratified analysis: a process of analysing smaller, more homogeneous subgroups according to specified criteria such as age groups, socioeconomic status, where there is variability (heterogeneity) in a population.

Subgroup: a defined set of individuals in a population group or of participants in a study such as subgroups defined by sex, age or risk status categories.

Subgroup analysis: an analysis in which the intervention effect is evaluated in a subgroup of a trial, including the analysis of its complementary subgroup. Subgroup analyses can be pre-specified, in which case they are easier to interpret. If not pre-specified, they are difficult to interpret because they tend to uncover false positive results.

Surrogate endpoint: a measure that is used in place of a primary endpoint (outcome). Examples are decrease in blood pressure as a predictor of decrease in strokes and heart attacks in hypertensive patients, and increase in T-cell (a type of white blood cell) counts as an indicator of improved survival of patients with AIDS. Use of a surrogate endpoint assumes that it is a reliable predictor of the primary endpoint(s) of interest.

Target population: in the context of a budget impact analysis the individuals with a given condition or disease who might avail of the technology being assessed within the defined time horizon.

Technology: the application of scientific or other organised knowledge – including any tool, technique, product, process, method, organisation or system – to practical tasks. In healthcare, technology includes drugs, diagnostics, indicators and reagents, devices, equipment and supplies, medical and surgical procedures, support systems,

and organisational and managerial systems used in prevention, screening, diagnosis, treatment and rehabilitation.

Time horizon: in the context of a clinical trial it is the time span over which patients are monitored for treatment effect.

Type I error: occurs when the null hypothesis is incorrectly rejected, also known as a false positive finding (See also **Type II error**).

Type II error: occurs when the alternative hypothesis is incorrectly rejected, also known as a false negative finding (See also **Type II error**).

Uncertainty: where the true value of a parameter or the structure of a process is unknown.

Validity: the extent to which an endpoint measures what it is intended to measure (See also **Reliability**).

Variability: this reflects known differences in parameter values arising out of inherent differences in circumstances or conditions. It may arise due to differences in patient population (e.g. patient heterogeneity – baseline risk, age, gender), differences in clinical practice by treatment setting or geographical location.

Appendices

Appendix 1 Further reading

Throughout the draft Guidelines key publications are cited as appropriate. However, a number of informative texts are available for more detailed treatments of some of the topics covered in these draft Guidelines.

Evaluating interventions:

Medical Research Council. Developing and evaluating complex interventions: new guidance. 2008. MRC (www.mrc.ac.uk/complexinterventionsguidance)

Patient relevant outcomes (PROs):

Brazier JE, Ratcliffe J, Tsuchiya A. et al. Measuring and valuing health for economic evaluation. 2007. Oxford: Oxford University Press

Brazier J, Yang Y, Tsuchiya A, Rowen D. A review of studies mapping (or cross walking) non-preference based measures of health to generic preference-based measures. The European Journal of Health Economics 2010;11(2):215

Brazier J, Roberts, J, Deverill, M. The estimation of a preference-based measure of health from the SF-36. J. Health Econ. 2002;21(2):271-92

Systematic reviews:

Higgins JPT, Green S (editors). Cochrane Handbook for Systematic Reviews of Interventions (Version 5.1.0). 2011. The Cochrane Collaboration (www.cochrane-handbook.org)

Meta-analysis techniques:

Egger M, Smith GD, Altman DG (editors). Systematic reviews in health care: meta-analysis in context. 2001. London, BMJ Publishing Group.

Wells GA, Sultan SA, Chen L, Khan M, Coyle D. Indirect Evidence: Indirect Treatment Comparisons in Meta-Analysis. 2009. Ottawa, Canadian Agency for Drugs and Technologies in Health.

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